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ABSTRACT

Thirteen papers presented at a conference on employment statistics and youth are contained in this report. Reviewed are the problems of gathering, interpreting, and applying employment and unemployment data relating to youth. The titles of the papers are as follow: "Counting Youth: A Comparison of Youth Labor Force Statistics in the Current Population Survey and the National Longitudinal Surveys"; "The Volatility of the Teenage Labor Market: Labor Force Entry, Exit, and Unemployment Flows"; "The Transition from School to Work with Job Search Implications"; "The Establishment of Stable and Successful Employment Careers: The Role of Work Attitudes and Labor Market Knowledge"; "Economic and Sociocultural Variables Affecting Rates of Youth Unemployment, Delinquency, and Crime"; "Racial Differentials in Male Youth Unemployment"; "The Effects of Child Labor Laws on Youth Unemployment"; "Youth Labor Markets and the Military"; "Direct Effects of Employment and Training Programs on Employment and Unemployment: New Estimates and Implications for Employment Policy"; "Social Development and Employment: An Evaluation of the Oakland Youth Experience Program"; "Control Group Selection"; "Methods of Allocating Funds to Alleviate Teenage Unemployment Problems"; and "Lowering Youth Unemployment: How Much and What Cost?" (JH)

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# Conference Report on Youth Unemployment: Its Measurement and Meaning

U.S. Department of Labor
Ray Marshall, Secretary

Office of the Assistant Secretary for Policy, Evaluation and Research
Arnold Packer, Assistant Secretary

Employment and Training Administration
Ernest G. Green
Assistant Secretary for Employment and Training

1978

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# INTRODUCTION

By: Robert Taggart and Naomi Berger Davidson

The statistical parameters of the youth employment problem are by now a familiar litany. Teenagers account for nearly a fourth of the unemployed, and persons under age 25 represent almost one half, even though they constitute only a tenth and a fourth of the labor force respectively. The rate of unemployment among youth age sixteen to nineteen is two and a half times the overall rate. Two-fifths of black teenagers in the labor force are without jobs.

There is general public recognition, based on such statistics, that youth unemployment is a critical national issue. Yet beyond this recognition, there is little agreement about the actual dimensions of the job deficit, the severity and implications of joblessness, its causes, or the effectiveness of alternative public policies. Libraries have been written about youth employment and unemployment, but the literature raises as many questions as it answers.

One fundamental reason is the inadequacy of our measurement tools and the uncertainties related to their application. The first step in addressing any problem in a "rigorous," "scientific" manner is to develop a set of measuring rods which will realistically appraise the characteristics of the phenomenon, which will aid in assessing its causes, and which will provide a baseline for determining the impacts of interventions. In the employment arena, the statistical system, while extremely sophisticated, was not developed for use in understanding and measuring youth employment problems. The concepts and definitions of "employment," "unemployment" and "labor force" participation were largely derived from the Depression experience, when the primary issue was joblessness among adult breadwinners. The meaning of these terms becomes clouded when applied to persons with very marginal and fluctuating attachments to the world of work. Interview methods by which the employment and unemployment data are derived may also bias results as they relate to youth, since the head of household who is interviewed may not know about the attitudes and activities of junior members. There are so many other factors involved during the "tempestuous teens" and early twenties that employment characteristics and developments cannot be considered alone.

By the same token, as youth have become an increasing share of the labor force, their inclusion in the overall employment and unemployment statistics have generated uncertainties about the meaning of these numbers. Equal weight is given a sixteen-year-old high school student looking for a 5-hour-a-week job to pay for clothes as is given the unemployed teacher with a family to support. The aggregate data mix "apples" and "oranges", and it is increasingly difficult to determine the implications of any given mixture. Some have argued, for instance, that historically high rates of national unemployment must be tolerated because so many youth are counted in the statistics whose problems are neither serious nor solvable.

A few illustrations quickly suggest some of the differences between adult and youth unemployment which cloud the overall data and make it difficult to assess the data which are available for youth:

- Half of teenagers (sixteen to nineteen) unemployed during the school year are students. This is a dramatic change from the early 1960s when less than a fourth of unemployed teenagers were in school. Among twenty to twenty-four-year-olds, only an eighth of the unemployed are students, and the proportion drops to 7% among twenty-five to thirty-four-year-olds.

- More than a third of sixteen to nineteen-year-olds are part-time job seekers, compared to a tenth of persons twenty-five and over. Teenagers who work average 28 hours of employment weekly compared to the 40-hour average of adults.

- Youth employment and unemployment is very seasonal. From December to July 1977, employment among sixteen to nineteen-year-olds rose by a fourth compared to a tenth for twenty to twenty-four-year-olds while remaining almost constant for persons twenty-five and over. Youth accounted for 95% of the December to July employment increase.

- Among persons with labor force experience during 1976, three-fifths of sixteen to seventeen-year-olds, two-fifths of eighteen to nineteen-year-olds, and a fifth of twenty to twenty-four-year olds were in the labor force less than half the year compared with only a tenth of older participants.

- Few young persons are breadwinners. Less than a tenth of sixteen to nineteen-year-olds in the civilian labor force are married with a spouse present, compared to two-fifths of twenty to twenty-four-year-olds and seven of every ten twenty-five to thirty-four-year-olds.

- The frequency of entry and exit from the labor force is a major factor in youth unemployment. In 1977, seven of ten unemployed youth were entrants or reentrants into the labor force, compared to less than two-fifths of all unemployed. Similarly, 53% of unemployed teenagers were jobless five weeks or less compared with only 38% of the unemployed age twenty and older.

- For every unemployed youth age sixteen to nineteen, there is another who claims to want a job but is not looking--largely because of school attendance.

- Among employed sixteen to nineteen-year-olds, more than a third earn less than the minimum wage. While some unemployed youth have unrealistic wage expectations, two-fifths claim they would take a job that paid less than the minimum and another fifth would take a job at the minimum. The mean wage for employed sixteen to nineteen-year-old males is less than half that for males twenty-five and over. For persons under age twenty with work experience, mean income in 1976 was only $1600 compared to $4900 for workers age twenty to twenty-four and $8300 for persons age twenty-five to twenty-nine.

- Youth with "serious" problems are a minority of all unemployed youth. Dropouts represent less than a fourth of unemployed sixteen to twenty-four-year-olds, and nonwhite school dropouts only one-twentieth. Only a third of unemployed youth are from poor families, and half from families below the BLS lower living standard. An eighth are from poor, nonwhite families. Likewise, only 6% of jobless sixteen to nineteen-year-olds are among the long-term unemployed (twenty weeks or more) compared to a fifth of adult unemployed.

- The labor force data for youth largely reflect demographic trends. The annual growth rate of the sixteen to nineteen labor force cohort was 3.9% from 1970 to 1975 compared to 2.2% for twenty-five to fifty-four-year-olds. From 1975 to 1980, the growth rate for youth is expected to slow to .8% annually. From 1980 to 1985, the youth labor force is expected to actively decline by 2.8% annually.
- From 1964 to 1970, government employment and training programs and the military accounted for one fourth of the employment growth (including Armed Forces) for sixteen to twenty-four-year-olds. From 1970 to 1976, the decline in the military exceeded the expansion in program enrollments, so that the government sector activity actually reduced employment growth.

These few examples suggest the many considerations in interpreting and applying employment and unemployment statistics regarding youth. The examples are evidence that some data are gathered on these important issues. However, the available statistics are in many cases sporadic and incomplete--adequate to highlight issues but not to address them fully. And in many cases, there are copious data but a great deal of uncertainty about what they mean.

To improve understanding of youth employment problems and programs, it is necessary to begin by improving our understanding of the measurement system, its uses and its abuses. A National Commission on Employment and Unemployment Statistics has been established by Congress to assess all aspects of our labor force statistics, including the problems in applying them to youth. Likewise, the Youth Employment and Demonstration Projects Act (YEDPA), signed by the President on August 5, 1977, provides the Secretary of Labor with discretionary authority to support experimental, demonstration and research activities which will lead to a better understanding of how to improve the employment and earnings of youth. One of the first priorities was, therefore, to address the issues related to the measurement of youth employment problems and program impacts.

In February 1978, a Conference on Employment Statistics and Youth was held to bring together the Nation's experts to assess these issues. The conference was organized by the Institute of Industrial Relations of the University of California, Los Angeles, under the joint sponsorship of the Department of Labor's Office of the Assistant Secretary for Policy, Evaluation and Research, and the Office of Youth Programs, with guidance from the National Commission on Employment and Unemployment Statistics. A total of twenty-two papers were commissioned for this conference. Based upon discussions at the conference and reviews by a panel of independent experts, the following thirteen papers were selected for inclusion in this volume. The remaining papers, which provide a range of important supplementary information, are available upon request from the Department of Labor's Office of Youth Programs.

The analyses included in this volume cover a number of interrelated topics. The first two papers focus on who is counted and the dynamic aspects underlying our static measures. In "Counting Youth: A Comparison of Youth Labor Force Statistics in the Current Population Survey and the National Longitudinal Surveys," Michael Borus, Frank Mott and Gilbert Nestel document how differences in survey techniques can make drastic differences in measured labor force status. The analysis documents the very wide margin in determining whether youth are really inside or outside the labor force, raising fundamental questions about the reliance which can be placed on fluctuations in measured employment and unemployment rates for youth. Ralph Smith and Jean Vanski, in "The Volatility of the Teenage Labor Market: Labor Force Entry, Exit, and Unemployment Flows," demonstrate that for youth even more than adults, the point-in-time measures of employment and unemployment mask an enormous flux as youth move into and out of jobs and the labor force. The "probabalistic" approach to analysis which considers these dynamic facts is complex but frequently is necessary to fully understand the static labor market statistics.

The next five papers examine labor market, institutional and societal factors which affect the measured rates of employment and unemployment. Stanley Stephenson's analysis, "The Transition from School to Work with Job Search Implications" examines the question whether unemployment during the early years in the labor force is constructive. One theory is that joblessness reflects an exploration process leading to better job opportunities. Another theory is that employment itself teaches job skills and leads to future success. The paper explores the evidence in favor of these theories. In "The Establishment of Stable and Successful Employment Careers: The Role of Work Attitudes and Labor Market Knowledge," Paul Andrisani examines the attitudinal and informational underpinnings of labor market participation by youths, addressing questions such as whether youth really want to work and how they learn and how much they know about labor market opportunities. Daniel Glaser's "Economic and Socioeconomic Variables Affecting Rates of Youth Unemployment, Delinquency and Crime" discusses the impact of unemployment on crime rates and the extent that illegal activities may provide an unreported alternative to gainful employment. Paul Osterman's paper on "Racial Differentials in Male Youth Unemployment" assessed all of the factors, including discrimination, which result in the staggeringly high levels of youth unemployment as well as the consequences of these severe problems. Finally, "The Effects of Child Labor Laws on Youth Employment" by Daniel Mitchell and John Clapp analyzes the legal impediments to youth employment and the way these affect measured rates of employment and unemployment.

The next four papers discuss the direct and indirect impacts of government activities. The military is a primary employer of youth, and Richard Cooper's "Youth Labor Markets and the Military" suggests the substantial and frequently ignored effect of Armed Forces requirements and policies on civilian employment and unemployment statistics. In "Direct Effects of Employment and Training Programs on Employment and Unemployment: New Estimates and Implications for Employment

Policy", Charles and Mark Killingsworth estimate the impacts of manpower program enrollments on the official employment and unemployment counts. "Social Development and Employment: An Evaluation of the Oakland Youth Work Experience Program" by Delbert Elliott and Brian Knowles explores the effects of a "model" work experience program on subsequent employment and earnings. While the results are not necessarily generalizeable, they give an indication of some possible considerations in assessing indirect impacts of employment and training programs on future employability. Ernst Stromsdorfer and Tei-Wei Hu discuss the ways in which program impacts on employment and earnings can be assessed in "Control Group Selection."

The final two papers focus on the use of employment and unemployment statistics in addressing questions about the scale of needed government initiatives and the distribution of resources. "Methods of Allocating Funds to Alleviate Teenage Unemployment Problems," by Joseph Cordes and Robert Goldfarb examines the formulae by which youth employment and training resources are distributed and the data bases to which these formulae are applied. Paul Flaim and Paul Ryscavage use available labor market statistics to estimate the job deficit for youth in "Lowering Youth Unemployment: How Much and at What Cost?"

Overall, then, this set of papers presents a panoramic review of the problems of gathering, interpreting and applying employment and unemployment data relating to youth. The conclusions are critically important in seeking to better understand youth employment problems and programs:

1. Survey data responses vary considerably depending on who is asked what question. Published Bureau of Labor Statistics information is frequently considered sacrosanct and used as a basis for highly sophisticated analysis. In fact, however, the youth unemployment rate may vary by more than half depending on who in a household is interviewed and how they are approached.

2. The youth labor market is highly dynamic, and static conceptualizations are not always appropriate. For instance, there is an obvious job deficit for youth, but increasing the number of jobs may simply bring new entrants into the labor force, or draw youth away from other openings, and might not directly reduce unemployment. If the jobs are more stable, this will reduce unemployment; if they are unstable they may increase shifts in and out of the labor force and employment. To the extent that occupational information and placement services lead to better job choices they will increase the stability of employment; to the extent they prolong the job search, they will raise unemployment.

3. In general, work experience seems to pay off in the sense that those who hold jobs during school tend to have more stable employment in the future. However, it is unknown whether government created slots which fill the job deficit are as effective as other employment, or whether the effects which prevail on the average also hold at the margin; i.e., whether putting another youth to work pays off in the future for that youth.

4. Youth in general do not have unrealistic job expectations, and like most everyone else, they respond in a reasonable fashion to labor market realities. Youth who know more about the labor market and who are more positively-oriented to work tend to do better. It is, again, uncertain whether public intervention can improve labor market knowledge or motivation, and whether the participants who are motivated and informed by such programs will do better than those who are not.

5. The interaction between crime and unemployment is apparently a two-way relationship. However, so many factors are involved that it is uncertain whether increased employment opportunities will lead to a direct reduction in crime.

6. Black youth employment problems are the result of a massive array of factors. Unemployment measures only one of the symptoms

and is not a very accurate reflection of differentials in opportunity. Unemployment does not have the same consequences for white youth as it does for blacks.

7. Youth under age eighteen are underrepresented in certain occupations covered by child labor laws. However, youth shift into these occupations as they age, and it is uncertain to what extent the laws alter overall youth employment or restrict certain groups of youth from getting into more promising career tracks. Likewise, it is unclear what risks are involved in the covered sectors and whether younger persons are better off outside.

8. The military is a major employer of youth. During the Vietnam era, growth of the Armed Forces absorbed large numbers of young men and contributed significantly to declining unemployment. During the 1970s, cutbacks in military personnel had the opposite effect. Despite the volunteer nature of military service, employment statistics continue to count the military sector separately. Inclusion might be more appropriate.

9. Employment and training programs have a direct effect on youth unemployment. It is estimated that expanding enrollments between 1964 and 1969 accounted for 90% of the decline in unemployment over this period. From 1969 to 1976, the programs did not expand any more rapidly than the youth labor force so their contribution to reducing unemployment was little greater in the latter years than in the former.

10. Indirect impacts of programs are extremely difficult to measure. Random assignment to experimental and control groups is really the only effective mechanism, and even this has such a wide margin of uncertainty that modest increases in employability from short-term youth programs are not easily registered.

11. Employment and training resources should be distributed according to needs and the types of problems being addressed. Because of the absence of unemployment data for youth by prime sponsor,

CETA funds must be distributed according to overall unemployment rates or poverty. These are poor proxies for youth unemployment and youth needs.

12. Available data cannot indicate how many youth would work if jobs were available. However, under a range of assumptions, it is possible to estimate the job deficit and wage bill necessary to meet it. To reduce teenage unemployment from 1976 to 1969 levels, assuming the new jobs would pay minimum wage, would cost around $1.7 billion. Economic recovery and the programs initiated under the Youth Employment and Demonstration Projects Act with a billion dollars of funding in fiscal 1978 can make a substantial dent in the problem.

These papers do not yield a coherent set of policy recommendations. Rather, they are suggestive of some of the deficiencies in youth employment statistics, some of the necessary considerations in their interpretations, and many of the issues involved in their application to assess program impacts.

The obvious recommendation is for more and better data concerning youth. Certainly, labor force information on youth by labor market area is necessary for the equitable distribution of resources. Much more needs to be gathered about the earnings of youth as well as the types of jobs they fill. Family status usually changes during the teen years and early twenties, and more information is required relating marital and family changes, as well as breadwinning responsibilities, to labor market changes. Some "hard" numbers are required on the income alternatives to work for youth and the activities of youth out of school and out of the labor force. The dynamic aspects of youth labor force participation suggest the need for more longitudinal information with monthly or even weekly, as opposed to annual, interviews in order to better determine the factors underlying employment status decisions. A determination must be made whether household head or individual interviews yield the more accurate information.

But the answer is not just more data. There are questions on how these numbers are used. The papers suggest some of the factors which must be considered in analyzing youth labor market experience. For instance, Armed Forces levels must be considered when looking at cyclical and secular trends in youth employment or unemployment. Certainly manpower program enrollments are a critical factor. Dynamic factors must be considered along with changes in the static measures. Efforts to assess employment and training programs must rely increasingly on random assignment control group selection techniques. Job deficit estimates from aggregate data must be tested by job guarantee experiments; reservation wage issues must, likewise, be tested to determine the willingness of youth to work under alternative conditions. In other words, there are so many considerations behind the youth employment experience that analysis will have to become increasingly sophisticated to expand the frontiers of knowledge. More data are needed, but better use can be and needs to be made of the data which are already available.

Until there are improvements in data and analytical approaches, it is necessary to fall back on some basic postulates and common sense notions. First, employment programs (including the military) can reduce the job deficit. The relationship between job creation and unemployment reductions is uncertain, but it is tantological that expanding enrollments will mean increasing employment when participants are counted as employed. The best guesses about the universe of need suggest that it is within the realm of possibility to drive down youth unemployment rates to tolerable levels by steady expansion of job creation efforts.

Second, almost all the measured effects will be the direct impacts on counted employment and unemployment. Indirect effects are difficult to determine because of the complexity of measuring net employment and earnings changes over time, as well as the impossibility of extensive demonstrations with randomly selected control groups. Also, gains realized in future years are good for the persons involved but do not change the status of youth; for instance, increased earnings at age twenty to twenty-four will not register for sixteen to nineteen year olds under programs directed to teenagers.

Third, lacking rigorous confirmation of the impacts of government interventions, there are at least some reasons for optimism. Work during the years of transition does seem to increase success probabilities; better youth jobs yield a better adaptation. Youth with more complete occupational information tend to have an easier time in the labor market. Employment provides an alternate to illegal activity even if the causal effects are difficult to pin down. In other words, the conventional wisdoms underlying youth policy--that more jobs are needed; that the quality of work and supervision matters, and that enrichment with occupational information and counseling makes sense--seem to be headed in the right direction. It remains to be determined whether government-created or accessed jobs have the same impacts as others in the labor market, whether or how much structured, supervised job settings make a difference, as well as the impacts of counseling and occupational information services. Given the difficulties of refined assessment, and the shortcomings of available data, these effects may never be determined convincingly. It is, therefore, necessary to move ahead based on best guesses, and the best guess is that common-sense notions are realistic.

Fourth, the problems of disadvantaged youth and minorities are not simple. The poor and minorities are worse off by almost every dimension; every factor which negatively affects all youth tends to affect them even more. On the other hand, there is evidence from most of the papers that positive actions and developments also have a magnified effect. Government programs and Armed Forces build-ups have their greatest impact on minority and low-income youth. The training received most benefits those who have no other options. It is disadvantaged youth who have the least information about the labor market and might be expected to benefit most from career counseling. Given the size of the minority and disadvantaged subgroups of the unemployed youth population, government interventions on a feasible scale can make a very substantial difference.

Finally, the papers in this volume suggest that we should not expect too much from "knowledge development" efforts. One of the declared purposes of the Youth Employment and Demonstration Projects Act is to determine "what works best for whom". Given the uncertainties of the data and their interpretation, it will be difficult to even assess aggregate YEDPA impacts on measured rates of unemployment much less to make refined estimates of incremental impacts of alternative interventions. The issues most likely to be answered clearly are those having to do with scale--how much is needed and how much can be done based on the experience of areas with more intensive funding--as well as those having to do with direct effects on aspirations, occupational awareness, motivation and the like, which can be assessed by entrance and termination tests. While sophisticated analysis of short- and long-term impacts on employability must be attempted--and randomly assigned control groups are needed whenever feasible--the fundamental uncertainities of data and the complications of youth labor market activities raise doubts about what can be proven in any rigorous manner. The "state of the art" in youth labor market statistics and interpretation of these statistics does not support overly sophisticated research and assessment (although this should not preclude attempts to use available data as rigorously as possible). It must be accepted that the shortcomings in employment statistics and their interpretation constrain what can be learned about youth employment problems and their solutions.

# COUNTING YOUTH:
# A COMPARISON OF YOUTH LABOR FORCE STATISTICS IN THE CURRENT POPULATION SURVEY AND THE NATIONAL LONGITUDINAL SURVEYS

By: Michael E. Borus, Frank L. Mott and Gilbert Nestel

## ABSTRACT

This study focuses on the accuracy of employment-related measures in the Current Population Survey (CPS) by comparing the CPS estimates with those of another survey--the National Longitudinal Surveys of Labor Force Behavior (NLS) which included approximately 5,000 young men and 5,000 young women between the ages of fourteen to twenty-four when the surveys began in 1966 and 1968, respectively. Since this paper is concerned with youth employment problems, the analysis is restricted to respondents between the ages of sixteen and twenty-one.

Differences between the CPS and NLS estimates of survey week employment-related behavior were found. The NLS labor force participation rates were significantly higher than those of the CPS, particularly among youth attending school. The NLS female unemployment rates were significantly higher than the CPS rates, while for the young men the NLS rates were slightly lower. The NLS data also showed a larger number of the unemployed seeking part-time employment than the CPS. The NLS found higher levels of employment, and among those at work the NLS youth were more likely to work part-time or overtime, depending on their ages. There was no discernible difference in the CPS and NLS estimates of mean hours worked by young women but the corresponding NLS estimates for the young men indicated a slightly higher work activity than the CPS.

The different estimates of the CPS and NLS could arise from differences in survey procedures. The authors believe that lack of self response in the CPS is the most likely explanation for the differences observed.

## INTRODUCTION

The focus of this paper is on the accuracy of the information gathered on youth by general population surveys such as the Current Population Survey (CPS).[1] Data accuracy is particularly important for

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1. This is a revised version of a paper presented to the Conference on Unemployment Statistics and Youth held at U.C.L.A. on February 11-12, 1978. We wish to thank Jean Haurin for her valuable help with this project.

the CPS since this is the primary source of national employment and unemployment statistics. This paper will examine the accuracy of employment-related variables in the CPS by comparing the CPS estimates with those of another survey--two cohorts of the National Longitudinal Surveys of Labor Force Behavior (NLS). The NLS samples included approximately 5,000 young men and 5,000 young women between the ages fourteen to twenty-four when the surveys began in 1966 and 1968, respectively.[2] Since this paper is concerned with youth employment problems we concentrate on the portions of the cohorts between the ages of sixteen and twenty-one. Therefore, the data analyzed are from the 1966, 1967 and 1968 NLS surveys of the young men, the 1968, 1969 and 1970 NLS surveys of the young women, and from tables published in Employment and Earnings for the CPS.[3] All data were gathered by the Census Bureau's CPS interviewers and the current labor force questions and coding were identical.

## DIFFERENCES IN SURVEY PROCEDURES

There were several differences between the two surveys. First, the NLS interviews the youth directly while the CPS seeks the information about the young person from the head of the household or some other responsible adult. In the majority of the cases the person interviewed by the CPS is a housewife who would most likely be the youth's mother. We have been unable to find much research which explores the effects of nonself response on labor force and employment status questions. An unpublished memorandum by Charles Jones and Robert Aquilino of the Census Bureau indicates that net differences in reports of employment status due to nonself response are not statistically different from zero at the 95 percent confidence level for all males and females, sixteen years of age and older.[4] A similar finding of nonsignificant differences for all adults occurs in the CPS-Census Match for 1970. The CPS-Census study, however, shows that there were significant differences for fourteen- to seventeen-year-olds, and the report goes on to note that there are noticeable differences by age, with the inconsistency dropping substantially as age increases.[5]

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[2] The data are gathered by the U.S. Bureau of the Census and analyzed by the Ohio State University under contract with the Office of Research and Development, ETA, U.S. Department of Labor. The views and opinions in this paper do not necessarily reflect those of any of the above agencies. For further information on the surveys see Herbert S. Parnes, et al., Career Thresholds, Manpower Research Monograph No. 16, vol. 1 (Washington: Government Printing Office, 1970) and John R. Shea, et al., Years for Decision, Manpower Research Monograph No. 24, vol. 1 (Washington: Government Printing Office, 1971).

[3] The reader should also see Parnes, et al., op. cit., Appendix E for an earlier comparison of the 1966 young men's survey and the October 1966 CPS.

Another difference between the CPS and NLS surveys is in the designation of the reference week. The CPS data refer to the specific week which includes the 12th of the month. The NLS data are gathered over a period of several months and refer to the week prior to the one in which the interview is conducted. Thus the CPS data are more likely to be affected by seasonal factors. For our comparisons we have selected CPS data for the month in which the NLS conducted the greatest number of interviews but we used all of the NLS respondents.

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[4] Charles Jones and Robert Aquilino, "Memorandum for Walter M. Perkins, Subject: Methods Test Phase III: Second Report on the Accuracy of Retrospective Interviewing and Effects of Nonself Response on Labor Force Status," unpublished memorandum within the Bureau of the Census, January 29, 1970.

[5] Bureau of the Census, Accuracy of Data for Selected Population Characteristics as Measured by the 1970 CPS-Census March, PHC(E)-11 (Washington: Government Printing Office, 1975), p.11 and Table 33. We should note that the poorer CPS-Census match for the youth may have been due to factors other than nonself response.

A third difference relates to the 1966 survey of the young men. Changes in the definitions of employment and unemployment were introduced in the CPS in January, 1967, but were used in the 1966 NLS coding. Thus CPS-NLS differences for that year may partly reflect these definitional differences. The definitions were identical for the other survey years of the young men and all of the surveys of the young women.

Differing ages at interview, particularly among the young men, also could cause variation between the reports of the two data sources. The NLS male sample consists of individuals who attained ages sixteen through twenty-one as of April 1 in the year of interview, whereas, the CPS includes individuals who were in that age group as of the survey month. Since the CPS data refer to November of each year, the NLS sample of the young men is approximately seven months older than the CPS group. In the case of the young women the age difference is considerably smaller. The NLS includes individuals who had attained the given ages as of January 1 of the interview year, while the CPS again uses the month of the interview. However, since the CPS data refer to January or February the age difference is small.

Finally, the longitudinal nature of the NLS may lead it to differ from the CPS. The NLS loses some of its sample from year to year. There is some evidence that there is more attrition among the unemployed,[6] but a multivariate analysis by the authors has shown this is not substantial. Most of the loss is attributable to young men entering the armed forces which removes them from the civilian population. These individuals are excluded from the CPS as well as the NLS. There also may be conditioning of the respondent's answers by repeated questioning. Such changes apparently occur in the CPS which finds different reports of employment status for different rotation groups.[7] Similar conditioning could occur in the later years of the NLS surveys.

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[6] See Frederick A. Zeller, et al., Career Thresholds, Manpower Research Monograph No. 16, Vol. 2, (Washington: Government Printing Office, 1971), Appendix A, and Roger D. Roderick, et al., Years for Decision, Manpower Research Monograph No. 24, Vol. 2 (Washington: Government Printing Office, 1973), pp. 15-20.

## RESULTS OF THE CPS-NLS COMPARISON

In this section we compare the levels and rates of labor force participation, employment, and unemployment; the number of hours of work being sought and the duration of unemployment for the unemployed; and the number of hours worked and occupational distributions for the employed as measured by the Current Population Survey and the National Longitudinal Surveys.

Labor force participation. Tables 1 and 2 present the CPS-NLS comparisons of the labor force participation rates for the young men and women, respectively. As is evident from these tables the labor force participation rates were significantly higher in the NLS for the total population and for the two race groups.[8] The CPS labor force participa-

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[7] Robert Pearl and Joseph Waksberg, "Effects of Repeated Household Interviews in the Current Population Survey," unpublished paper presented at the 47th National Conference of the American Marketing Association, Dallas, Texas, June 17, 1964.

[8] Our use of the word significant means that we have rejected the null hypothesis of equality of proportions in the two surveys. Each of the statistical tests used a two-tail criterion, the type 1 error was 5 percent and the standard error of the estimator was increased by 1.4 to reflect the complex design of the two surveys. The CPS sample is self weighting with each respondent representing 1,200 other individuals in the universe. The estimated sample sizes were obtained by dividing the corresponding universe totals by 1,200. The actual number of sample cases was used for the NLS. We were unable to make tests of significance of the levels presented in Tables 3-6 since we did not have the variances for the CPS data. Comparisons of the two surveys for this information is descriptive.

## TABLE 1

## CPS AND NLS COMPARISON OF LABOR FORCE PARTICIPATION RATES AND UNEMPLOYMENT RATES OF YOUNG MEN 16 TO 21 YEARS OF AGE, BY RACE AND SCHOOL STATUS, SURVEY WEEKS 1966 TO 1968

| Characteristic | 1966[a] | | | | 1967 | | | | 1968 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | CPS | | NLS[d] | | CPS[e] | | NLS[f] | | CPS[g] | | NLS[h] | |
| | LFPR | UR | LFPR | UR | LFPR | UR | LFPR | UR | LFPR | UR | LFPR | UR |
| Total | 49.8[b] | 10.3[c] | 68.4 | 12.6 | 55.8 | 11.4 | 70.4 | 10.5 | 54.8 | 9.6 | 70.4 | 9.0 |
| White | | | | | 55.8 | 10.0 | 69.9 | 9.3 | 55.1 | 8.2 | 70.1 | 8.3 |
| Nonwhite | | | | | 55.9 | 20.7 | 73.4 | 17.9 | 52.5 | 18.9 | 72.8 | 13.8 |
| Major activity was school | | | | | 36.5 | 13.1 | 53.3 | 17.1 | 37.1 | 12.2 | 53.3 | 14.3 |
| Major activity not school | | | | | 91.3 | 10.2 | 93.8 | 4.8 | 90.7 | 14.4 | 92.9 | 4.3 |

a Figures for 1966 only include men 16 to 19 years of age.
b December 1966 from Employment and Earnings, Vol. 13, No. 7, January 1967.
c November 1966 from Employment and Earnings, Vol. 13, No. 6, December 1966.
d Survey conducted October 1966 to February 1967.
e November 1967 from Employment and Earnings, Vol. 14, No. 6, December 1967.
f Survey conducted October 1967 to January 1968.
g November 1968 from Employment and Earnings, Vol. 15, No. 6, December 1968.
h Survey conducted October 1968 to January 1969.

## TABLE 2

## CPS AND NLS COMPARISON OF LABOR FORCE PARTICIPATION RATES AND UNEMPLOYMENT RATES OF YOUNG WOMEN 16 TO 21 YEARS OF AGE, BY RACE AND SCHOOL STATUS, SURVEY WEEKS 1968 TO 1970

| Characteristic | 1968 | | | | 1969 | | | | 1970 | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | CPS[a] | | NLS[b] | | CPS[c] | | NLS[d] | | CPS[e] | | NLS[f] | |
| | LFPR | UR | LFPR | UR | LFPR | UR | LFPR | UR | LFPR | UR | LFPR | UR |
| Total | 43.8 | 11.7 | 50.7 | 15.8 | 42.8 | 9.2 | 53.2 | 15.1 | 45.6 | 11.6 | 54.5 | 16.9 |
| White | 44.9 | 10.5 | 50.8 | 14.6 | 43.4 | 7.8 | 53.6 | 13.6 | 47.1 | 10.1 | 55.3 | 15.5 |
| Nonwhite | 37.0 | 21.3 | 49.8 | 24.1 | 39.2 | 19.2 | 50.4 | 25.8 | 36.4 | 23.1 | 49.3 | 27.3 |
| Major activity was school | 27.1 | 12.2 | 35.3 | 19.7 | 26.0 | 7.9 | 39.9 | 19.2 | 29.3 | 11.5 | 41.7 | 28.9 |
| Major activity not school | 62.3 | 11.4 | 65.0 | 13.4 | 61.7 | 9.8 | 64.6 | 12.7 | 63.2 | 11.6 | 67.0 | 12.5 |

a February 1968, from Employment and Earnings, Vol. 14, No. 9, March 1968.
b Survey conducted January 1968 to May 1968.
c January 1969, from Employment and Earnings, Vol. 15, No. 8, February 1969.
d Survey conducted December 1968 to March 1969.
e February 1970, from Employment and Earnings, Vol. 16, No. 9, March 1970.
f Survey conducted, January 1970 to March 1970.

tion rates for young men, sixteen-to twenty-one-year-olds, were approximately 55%, while the corresponding NLS rates were about fifteen percentage points higher. Table 3 translates the NLS labor force participation rates into estimates of the labor force using as the base the CPS estimates of the civilian noninstitutional population. The result is a labor force estimate that includes approximately 1.3 to 1.5 million more young men aged sixteen to twenty-one than was found in the CPS.[9]

The major activity of the youth during the survey week helps to explain the large difference between the two surveys.[10] Among the young men attending school the NLS labor force participation rates were approximately sixteen percentage points higher, while there was no significant difference between surveys in these rates for those young men not in school.

Similar findings occurred among the young women, sixteen to twenty-one years of age. Again the NLS found significantly higher labor force participation rates than the CPS but the differences (between 7 and 10 percentage points) were smaller than for the young men's cohort. Still, based on CPS population figures the NLS found approximately .7 to 1.1 million more labor force participants than the CPS (Table 4). As was the case for the young men, reporting of labor force participation rates was significantly higher in the NLS among the young women whose major activity was school during the survey week. The differences between the rates were three to almost five times larger for the in-school women than for those who had some other major activity (Table 2).

Unemployment. Among young men who were sixteen to twenty-one, there was very little difference in the overall unemployment rate between the two sources of data. This was in part due to offsetting differences;

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[9] We implicitly assume that the attrition from the NLS does not affect the labor force participation rates.

[10] Major activity is defined by the survey respondent in his or her answer to the question "What was...doing most of last week--Working, Keeping house, Going to school, or something else?"

TABLE 3

CPS AND NLS COMPARISON OF THE NUMBER (IN THOUSANDS) OF YOUNG MEN 16 TO 21 YEARS OF AGE IN THE LABOR FORCE, BY EMPLOYMENT STATUS AND RACE, SURVEY WEEKS 1967 AND 1968

| Characteristic | 1967[a] | | | | | | 1968[a] | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | CPS[b] | | | NLS[c] | | | CPS[b] | | | NLS[c] | | |
| | Total | White | Nonwhite | Total | White | Nonwhite | Total | White | Nonwhite | Total | White | Nonwhite |
| Civilian noninstitutional population | 9,009 | 7,821 | 1,188 | 9,009 | 7,821 | 1,188 | 9,349 | 8,115 | 1,234 | 9,349 | 8,115 | 1,234 |
| In labor force | 5,031 | 4,367 | 664 | 6,342 | 5,467 | 872 | 5,121 | 4,473 | 646 | 6,582 | 5,689 | 898 |
| Employed | 4,458 | 3,931 | 527 | 5,676 | 4,958 | 716 | 4,631 | 4,105 | 526 | 5,989 | 5,217 | 774 |
| Employed-major activity school | 1,849 | 1,706 | 143 | 2,381 | 2,159 | 218 | 2,041 | 1,884 | 157 | 2,454 | 2,195 | 256 |
| Employed-major activity not school | 2,608 | 2,224 | 384 | 3,296 | 2,799 | 498 | 2,590 | 2,221 | 369 | 3,536 | 3,023 | 513 |
| Unemployed | 574 | 437 | 137 | 666 | 508 | 156 | 491 | 368 | 122 | 593 | 472 | 124 |
| Unemployed-major activity school | 279 | 231 | 48 | 489 | 394 | 101 | 284 | 215 | 69 | 428 | 339 | 91 |
| Unemployed-major activity not school | 295 | 206 | 89 | 166 | 114 | 55 | 206 | 153 | 53 | 165 | 133 | 33 |
| Unemployed-seeking full time work | 289 | 199 | 89 | 170 | 118 | 52 | 208 | 156 | 51 | 156 | 117 | 40 |
| Unemployed-seeking part time work | 285 | 238 | 48 | 496 | 390 | 104 | 283 | 212 | 71 | 437 | 355 | 84 |

[a] Totals may not equal sum of parts due to rounding.

[b] For data sources see footnotes of Table 1.

[c] The NLS calculations apply NLS rates from Table 1 to CPS totals for the civilian noninstitutional population.

TABLE 4

CPS AND NLS COMPARISON OF NUMBER (IN THOUSANDS) OF YOUNG WOMEN 16 TO 21 YEARS OF AGE IN THE LABOR FORCE, BY EMPLOYMENT STATUS AND RACE, SURVEY WEEKS 1968 TO 1970

| Characteristic | 1968[a] | | | | | | 1969[a] | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | CPS | | | NLS[b] | | | CPS | | | NLS[b] | | |
| | Total | White | Nonwhite | Total | White | Nonwhite | Total | White | Nonwhite | Total | White | Nonwhite |
| Civilian noninstitutional population | 10,405 | 9,041 | 1,364 | 10,405 | 9,041 | 1,364 | 10,622 | 9,201 | 1,422 | 10,622 | 9,201 | 1,422 |
| In labor force | 4,559 | 4,055 | 504 | 5,275 | 4,593 | 679 | 4,550 | 3,994 | 557 | 5,651 | 4,932 | 717 |
| Employed | 4,026 | 3,629 | 397 | 4,442 | 3,922 | 516 | 4,131 | 3,681 | 450 | 4,798 | 4,263 | 532 |
| Employed-major activity school | 1,295 | 1,217 | 78 | 1,467 | 1,309 | 156 | 1,342 | 1,249 | 93 | 1,603 | 1,456 | 145 |
| Employed-major activity not school | 2,731 | 2,412 | 319 | 2,975 | 2,613 | 360 | 2,788 | 2,432 | 357 | 3,196 | 2,807 | 387 |
| Unemployed | 534 | 426 | 108 | 834 | 671 | 164 | 419 | 313 | 107 | 853 | 671 | 185 |
| Unemployed-major activity school | 180 | 149 | 31 | 365 | 316 | 50 | 115 | 85 | 30 | 383 | 327 | 57 |
| Unemployed-major activity not school | 353 | 276 | 77 | 469 | 355 | 114 | 305 | 228 | 77 | 470 | 344 | 128 |
| Unemployed-seeking full time work | 331 | 261 | 70 | 410 | 307 | 103 | 278 | 207 | 71 | 403 | 292 | 112 |
| Unemployed-seeking part time work | 203 | 165 | 38 | 424 | 364 | 61 | 141 | 106 | 36 | 450 | 378 | 73 |

(Table continued on next page.)

TABLE 4 continued

| Characteristic | 1970 [a] | | | | | |
|---|---|---|---|---|---|---|
| | CPS | | | NLS[b] | | |
| | Total | White | Nonwhite | Total | White | Nonwhite |
| Civilian noninstitutional population | 10,755 | 9,275 | 1,480 | 10,755 | 9,275 | 1,480 |
| In labor force | 4,905 | 4,366 | 539 | 5,861 | 5,129 | 730 |
| Employed | 4,338 | 3,924 | 414 | 4,871 | 4,334 | 530 |
| Employed-major activity school | 1,446 | 1,358 | 88 | 1,732 | 1,589 | 139 |
| Employed-major activity not school | 2,891 | 2,565 | 326 | 3,139 | 2,744 | 391 |
| Unemployed | 567 | 442 | 125 | 991 | 795 | 199 |
| Unemployed-major activity school | 188 | 149 | 40 | 542 | 466 | 77 |
| Unemployed-major activity not school | 379 | 294 | 85 | 449 | 329 | 122 |
| Unemployed-seeking full time work | 349 | 268 | 81 | 506 | 382 | 126 |
| Unemployed-seeking part time work | 218 | 174 | 44 | 485 | 413 | 73 |

a Totals may not equal sum of parts due to rounding.

b For data sources see footnotes of Table 2.

c The NLS calculations apply NLS rates from Table 2 to CPS totals for the civilian noninstitutional population.

the NLS had higher unemployment rates for those youth who listed their major activity as school,[11] while the CPS had significantly higher unemployment rates for young men with another major activity. The CPS also had significantly higher unemployment rates than the NLS for nonwhite youth in 1968.[12]

The women in the NLS reported significantly higher unemployment rates for the entire group and for those attending school. For the three years studied, the NLS reported between 300,000 and 434,000 more unemployed young women than did the CPS. These were increases of 56% to 104% in the number of young women who were classified as unemployed. Due to the substantially higher unemployment rates in the NLS, approximately 40% of the increased labor force participants found among young women by the NLS survey were unemployed (Table 4).

Approximately 100,000 more young men were classified as unemployed in the NLS than in the CPS as a result of the higher labor force participation rates in the former survey. Yet, as is seen in the last two rows of Table 3, the CPS reported substantially more young men seeking full-time employment. Whereas about half of the CPS sample said they were looking for full-time work, only 25% of the NLS sought a full-time job. This difference could be due to the larger number of unemployed in the CPS sample who did not list school as their major activity in the survey

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11. The difference was statistically significant in 1967 but was not in 1968.

12. The NLS sample did not interview persons who were in the armed forces at the time of the first survey but who returned to civilian life in a subsequent year. To the extent that these veterans are more likely to participate in the labor force or to experience unemployment the corresponding NLS rates would be lower than the CPS rates. Since the NLS labor force participation rates exceeded the CPS estimates we feel that this difference in survey design is not important for this variable. The lower unemployment rate in the NLS for nonwhite youth, however, could be caused by this difference.

week. (We calculated that the CPS contained 50 to 130 thousand more unemployed young men who said that something other than school was their major activity.)[13] Almost all of these individuals wanted full-time jobs. On the other hand, the CPS had many fewer unemployed whose major activity was school and who were primarily interested in part-time jobs. Those answering that school was their major activity had much lower labor force participation and somewhat lower unemployment rates in the CPS (Table 1).

Similar to the men, there were substantially more unemployed young women seeking part-time work in the NLS than in the CPS; however, the NLS indicated more young women seeking full-time employment as well (Table 4). The higher labor force participation and unemployment rates in the NLS for women reporting school and women reporting some other major activity in the survey week account for the substantially greater numbers of unemployed women in the NLS seeking both full-time and part-time jobs.

There were no discernible differences in the reports by the young men in the CPS and NLS on duration of unemployment. Approximately the same proportions in both surveys reported being unemployed for less than five weeks and fifteen or more weeks. The young women in the NLS, on the other hand, reported a considerably shorter duration of unemployment during 1968 and 1970, but had somewhat lower percentages reporting short periods of unemployment during 1969. This difference may have been due to the use of January data for the CPS in 1969 and February data for 1968 and 1970. Because of the large month-to-month variation in the CPS reports of duration we hesitate to draw any conclusion.

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[13] The CPS and NLS had about the same labor force participation rates for this group but the unemployment rate was much higher in the CPS.

Employment. The NLS found significantly higher levels of employment than did the CPS--about 25% more employed young men and 10% more employed young women. For example, the NLS estimates of youth employment in 1968 exceeded those of the CPS by approximately 1.8 million, of whom roughly three-quarters were young men. The differences in employment were somewhat more prevalent among the nonwhite segments of both NLS samples and among those persons listing their major activity as something other than school.

The distribution of hours worked during the survey week also was substantially different in the two surveys. The NLS found more part-time workers and workers employed overtime (in excess of 40 hours) than did the CPS (Tables 5 and 6). The number of sixteen to twenty-one year old youth employed for more than 40 hours was from 50% to 100% greater in the NLS than in the CPS.

The differences in the two survey's reports of the number of hours worked by youth were related to the age of the respondents. Workers who were sixteen and seventeen years of age were more likely to be working only part-time. Therefore, of the additional workers in these ages reported by the NLS, approximately 55% of the young men and 85% of the young women were employed less than 35 hours a week. On the other hand, the older youth were more likely to include persons working overtime, and the NLS found many more eighteen- to twenty-one-year-olds working for over 40 hours per week as compared with the CPS. In some cases the difference in the number working overtime exceeded the total numerical differences between the two samples for this age group. Finally, since the NLS reported more youth working overtime and a slightly smaller percentage working part-time (particularly among the men), the NLS found somewhat higher mean hours of work for the entire sample and for workers on full-time schedules. The differences were more pronounced among the young men, probably reflecting the smaller percentage working part-time.[14]

## SUMMARY AND CONCLUSIONS

We have found that the NLS when compared to the CPS reports:

1) Significantly higher labor force participation among young men and women, particularly among those whose major activity is attending school. These differences occurred in both white and nonwhite groups.
2) Significantly higher unemployment rates for young women and approximately the same rates for young men. For both young men and women the number of unemployed is higher.
3) More of the unemployed are seeking part-time employment.
4) Considerably higher levels of employment, particularly for the young men.
5) The youth are more likely to work either part-time or overtime depending on their age, and mean hours worked by the young men in the survey week are somewhat higher.

Obviously, we cannot say conclusively that the NLS reports are more accurate than those of the CPS in the light of the differences in the two surveys mentioned earlier. If, however, the NLS is correct these findings have significant implications. For 1968, the CPS youth labor force would have been understated by almost one-fourth or nearly 2.2 million young men and women. Employment would have been approximately 1.75 million higher and unemployment would have increased by 400,000 (an increase of almost 40 percent over the CPS reported number). This would mean that there was a sizable "undercount" by the CPS.

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[14] Finally, the single digit occupational distributions of the two samples were very similar. There is no evidence that the additional workers reported in the NLS were concentrated in any particular occupational group.

Although obviously somewhat biased judges, we tend to believe the NLS estimates. The pattern of reported differences appears consistent over time, tending to negate the possibility that the longitudinal nature of the studies or the difference in definitions during 1966 leads to the differences in findings for the two surveys. The fact that the average NLS respondent was seven months older than his CPS counterpart, while conceivably causing some of the observed differences, could not have accounted for all of the greater labor force participation found in the NLS. As a check, we reran sections of Table 1 restricting the NLS sample to young men fifteen through twenty years of age and compared the findings with the CPS results for men sixteen through twenty-one years old. Even though the NLS sample was now younger and the difference between the two surveys narrowed, we still found higher labor force participation for the NLS.[15] We also conducted some analyses which restricted the NLS sample to interviews collected in the same month as the CPS data. The results of such a restriction on the NLS data did not appear to make sizeable differences in the estimates of the employment-related variables.

At the same time, the nature of the differences we observed between the two samples is consistent with what one would expect due to problems of nonself response. When the youth are in school one could expect that their mothers would consider them out of the labor force. The mother would tend to disregard or be unaware of part-time employment and might not even know of sporadic attempts by their children to look for employment. It is also quite possible that the mother would not know of overtime work in the survey week and would report the standard full-time schedule. Finally, for those older youths who are only tangentially attached to the household (e.g., they are away at college or more in and out of the household depending on their financial state and familial

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15. We were also able to use the age attained at survey month for the young women in some special runs. These too did not noticeably alter the conclusion.

relationships), the mother may have no idea of their employment status.[16]

While our leanings are toward the NLS data, there are at least two possibilities for testing the accuracy of the data sets. First, the Census Bureau could expand its Methods Test Panel and seek a larger sample of young self respondents to reinterview after another member of the household has provided labor force data. The expansion would have to be substantial, however, in order to have a large enough sample in this limited age group.

Second, if the NLS estimates are more accurate and there are more youth in the labor force seeking employment than the CPS shows, there may be some indirect evidence which we can observe over the next year. The new youth programs under the Youth Employment and Demonstration Projects Act of 1977 (YEDPA) will provide roughly 200,000 additional youth slots.[17] If the CPS is correct the filling of these slots would come primarily from among the unemployed. On the other hand, if many of the people the CPS says are out of the labor force are really seeking work as the NLS implies, the slots will be filled without having much impact on the CPS measure of youth unemployment. We should be prepared for the YEDPA programs to "fail" to lower unemployment if in fact we are presently not counting youth correctly.

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16. The fact that we found smaller differences in labor force participation rates among the young women is also consistent with the nonself response hypothesis, since these individuals are more likely than their male counterparts to be in their own household and, thus, are more likely to be reporting for themselves.

17. The estimate is very inexact since it is not clear how the CETA prime sponsors will divide their funds between in-school and out-of-school programs.

## TABLE 5

## CPS AND NLS COMPARISON OF THE NUMBER (IN THOUSANDS) OF YOUNG MEN AT WORK IN NONAGRICULTURAL INDUSTRIES, BY HOURS WORKED AND AGE, SURVEY WEEKS 1967 AND 1968[a]

| Characteristic | 1967 | | 1968 | |
|---|---|---|---|---|
| | CPS[b] | NLS[c] | CPS[b] | NLS[c] |
| Total at work | | | | |
| 16-17 | 1,064 | 1,618 | 1,109 | 1,726 |
| 18-19 | 1,396 | 1,647 | 1,565 | 1,956 |
| 20-21 | 1,576 | 1,753 | 1,531 | 1,619 |
| 16-21 | 4,036 | 5,018 | 4,205 | 5,301 |
| On part time schedule | | | | |
| 16-17 | 883 | 1,183 | 947 | 1,284 |
| 18-19 | 555 | 600 | 665 | 681 |
| 20-21 | 351 | 395 | 382 | 357 |
| 16-21 | 1,789 | 2,178 | 1,994 | 2,322 |
| On full time schedule | | | | |
| 16-17 | 176 | 435 | 162 | 442 |
| 18-19 | 841 | 1,047 | 900 | 1,275 |
| 20-21 | 1,230 | 1,358 | 1,150 | 1,262 |
| 16-21 | 2,247 | 2,840 | 2,211 | 2,979 |
| Working over 40 hours | | | | |
| 16-17 | 50 | 146 | 39 | 180 |
| 18-19 | 277 | 519 | 290 | 687 |
| 20-21 | 468 | 690 | 389 | 654 |
| 16-21 | 795 | 1,355 | 718 | 1,521 |
| Mean hours, all at work | | | | |
| 16-17 | 19 | 23 | 19 | 24 |
| 18-19 | 33 | 35 | 31 | 35 |
| 20-21 | 40 | 41 | 39 | 41 |
| 16-21 | 31 | 32 | 30 | 33 |
| Mean hours, those on full time schedule | | | | |
| 16-17 | 41 | 42 | 40 | 43 |
| 18-19 | 43 | 45 | 41 | 45 |
| 20-21 | 44 | 46 | 43 | 45 |
| 16-21 | 43 | 44 | 42 | 45 |

[a] Totals may not equal sum of parts due to rounding.

[b] For data sources see footnotes of Table 1.

[c] The NLS calculations apply NLS rates from Table 1 to CPS totals for the civilian noninstitutional population.

## TABLE 6

## CPS AND NLS COMPARISON OF THE NUMBER (IN THOUSANDS) OF YOUNG WOMEN AT WORK IN NONAGRICULTURAL INDUSTRIES, BY HOURS WORKED AND AGE, SURVEY WEEKS 1968 TO 1970[a]

| Characteristics | 1968 | | 1969 | | 1970 | |
|---|---|---|---|---|---|---|
| | CPS[b] | NLS[c] | CPS[b] | NLS[c] | CPS[b] | NLS[c] |
| Total at work | | | | | | |
| 16-17 | 773 | 1,035 | 789 | 1,209 | 969 | 1,272 |
| 18-19 | 1,462 | 1,583 | 1,419 | 1,538 | 1,486 | 1,510 |
| 20-21 | 1,647 | 1,618 | 1,773 | 1,740 | 1,726 | 1,877 |
| 16-21 | 3,882 | 4,236 | 3,981 | 4,487 | 4,181 | 4,659 |
| On part time schedule | | | | | | |
| 16-17 | 708 | 916 | 730 | 1,121 | 859 | 1,112 |
| 18-19 | 515 | 579 | 532 | 563 | 602 | 598 |
| 20-21 | 374 | 369 | 400 | 433 | 476 | 480 |
| 16-21 | 1,598 | 1,864 | 1,662 | 2,118 | 1,936 | 2,190 |
| On full time schedule | | | | | | |
| 16-17 | 65 | 120 | 59 | 88 | 110 | 160 |
| 18-19 | 947 | 1,004 | 887 | 977 | 884 | 912 |
| 20-21 | 1,273 | 1,248 | 1,373 | 1,299 | 1,250 | 1,397 |
| 16-21 | 2,284 | 2,372 | 2,319 | 2,365 | 2,245 | 2,469 |
| Working over 40 hours | | | | | | |
| 16-17 | 12 | 46 | 11 | 22 | 20 | 38 |
| 18-19 | 143 | 230 | 126 | 234 | 110 | 228 |
| 20-21 | 199 | 313 | 226 | 277 | 176 | 344 |
| 16-21 | 353 | 589 | 362 | 534 | 306 | 610 |
| Mean hours, all at work | | | | | | |
| 16-17 | 14 | 15 | 14 | 14 | 15 | 16 |
| 18-19 | 31 | 31 | 31 | 30 | 30 | 30 |
| 20-21 | 36 | 36 | 36 | 35 | 35 | 36 |
| 16-21 | 29 | 29 | 29 | 27 | 28 | 28 |
| Mean hours, those on full time schedule | | | | | | |
| 16-17 | 38 | 39 | 36 | 39 | 38 | 37 |
| 18-19 | 39 | 40 | 40 | 39 | 39 | 40 |
| 20-21 | 40 | 41 | 40 | 39 | 39 | 40 |
| 16-21 | 40 | 40 | 40 | 39 | 39 | 40 |

[a] Totals may not equal sum of parts due to rounding.

[b] For data sources see footnotes of Table 2.

[c] The NLS calculations apply NLS rates from Table 2 to CPS totals for the civilian noninstitutional population.

# THE VOLATILITY OF THE TEENAGE LABOR MARKET: LABOR FORCE ENTRY, EXIT, AND UNEMPLOYMENT FLOWS

By: Ralph E. Smith and Jean E. Vanski

## ABSTRACT

Gross flow tabulations from the Current Population Survey provide estimates of the number of people who move between unemployment, employment, and non-participation in the labor force from one month to the next. This paper describes these data and their potential value for analyzing labor market dynamics and uses the data for teenagers to analyze recent cyclical, trend, and seasonal patterns in their unemployment and labor force participation rates.

It is shown that the gross flow series provide important information about the youth labor market, supplementing the information obtained from conventional labor force series. Analysis of data from the 1967-1977 period suggests, for example, that the chronically high unemployment rate of teenagers is directly linked to their large flows through the labor market. In addition, the deterioration in the relative position of black teenagers during this period is a result of both secular and cyclical differences in their behavior. Not only are their rates of labor force entry failing to keep pace with those of white teenagers, but also their success in finding work is decreasing more rapidly, further decreasing their labor force participation. The widening gap between the unemployment rates of the two groups directly contributes to the widening gap in their participation rates.

## INTRODUCTION

High unemployment rates among teenagers, especially black teenagers, are given considerable attention by economic policymakers and analysts. Debates often focus on the extent to which the rates really reflect a serious problem and, if they do, what can be done to reduce that problem. Discussions of teenage unemployment inevitably lead to consideration of the high turnover and labor force mobility of youth. Those who minimize the seriousness of youth unemployment point out that much of it is frictional, resulting from teenagers moving in and out of the labor force. The special job needs of many teenagers, as they combine school and work, make it more difficult to design programs to reduce their unemployment.

The dynamic character of the youth labor market is not reflected very well in the conventional unemployment tabulations from the Current Population Survey (CPS) published by the Department of Labor. These data series provide the net changes from one month to the next in unemployment, employment, and non-participation, but give little indication of the much larger gross movements that normally occur between each of the three labor market states. For example, during 1976 the published monthly series suggested a fairly static labor market in which between 18% and 20% of teenagers who participated in the labor force were unemployed. But underlying this static picture was a labor force in a constant state of change: each month about half of all unemployed teenagers either found jobs or left the labor force, only to be replaced by a roughly equal number of teenagers who previously were outside the labor force or were employed.

A little-used source of information about these labor market dynamics is the unpublished gross flow series from the CPS, maintained by the Bureau of Labor Statistics. Based on the responses of individuals who were in the CPS sample in consecutive months, these tabulations provide estimates of the number of people who move between unemployment, employment, and non-participation in the labor force each month. Previously, we have used these gross flow data, disaggregated by age, race, and sex, to estimate a monthly model of the U.S. labor market and to simulate the impact of alternative macroeconomic time paths on the labor market status of each group.[1] For this conference, we have updated the equations that go into this model for teenagers to reflect the experience of the recent recession and the beginning of the recovery.

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[1] The model simulates the labor force behavior of sixteen demographic groups including white males, white females, nonwhite males, and nonwhite females, each disaggregated by age groups (16-19; 20-24; 25-59; and 60 and over). Blacks comprise about 90% of the nonwhite categories. For a description of this model, see Ralph E. Smith, "A Simulation Model of the Demographic Composition of Employment, Unemployment, and Labor Force Participation," in Ronald G. Ehrenberg, Research in Labor Economics, vol. 1 (1977), pp.259-303.

Our paper has two purposes. First, we seek to alert analysts to the potential value (and limitations) of the CPS gross flow data for understanding the teenage labor market. Second, we wish to report some of our own research, which uses these data to analyze recent cyclical, trend, and seasonal patterns of teenage unemployment and labor force participation.

Since the gross flow data are not widely known, the first section presents a description of the data, its problems, and previous uses. We have found it necessary to adjust the data because of inconsistencies with the net changes in the conventional labor market series. Our adjustment technique and the size of these adjustments for teenagers are reported. The second section presents new estimates of the cyclical, secular, and seasonal patterns in the labor market activities of teenagers, by race and sex. The final section presents our conclusions and recommendations.

## LABOR FORCE GROSS FLOW DATA

The analysis reported in this paper is based on data describing gross changes in the labor force. The data are derived from the Current Population Survey (CPS) which provides the Bureau of Labor Statistics' monthly estimates of labor force status. Their value rests in their ability to complement the information on monthly changes in labor force levels coming from the full CPS with information on short run market dynamics. This section of the paper will describe the derivation of the gross flow data and their potential value for research on labor market behavior; it will discuss reported errors in the data which have limited their use; and, finally, it will provide a description of a technique which has been developed to compensate for these errors.

### Derivation of Gross Flow Data

Each month approximately 56,000 households are interviewed in the Current Population Survey to determine the labor force status of the civilian non-institutional population aged sixteen and over.

The CPS is constructed as a panel survey with each household remaining in the sample for four consecutive months, being removed for the next eight months, and reentering for another four months. As the households rotate in and out of the sample, potentially 75% of them are common from month-to-month. These households form the basis for gross flow data.[2]

Since individuals within these households are in the sample for two consecutive months, it is possible to match their current and previous labor force status. Aggregating data on individuals into civilian population groups, $P_i$, the gross change tabulations provide estimates of the size of flows between those who were employed, $E_i$, unemployed, $U_i$, and not in the labor force, $N_i$. Throughout the remainder of this paper, the gross flow data will be denoted by an ordered pair of letters representing preceding and current month's labor force status. For example, $UN_i$, will denote those people in the $i^{th}$ population group who were unemployed last month, but left the labor force in the current month.

The gross change data provide information on the degree of mobility in the labor market as people enter or leave the labor force or move between employment and unemployment. This is particularly valuable for population groups with relatively weak labor force attachment, such as teenagers. Figure 1 illustrates this point by showing the average 1976 gross flows for sixteen to nineteen-year olds which help generate teenage levels of employment, unemployment and nonlabor force status as reported by the CPS.

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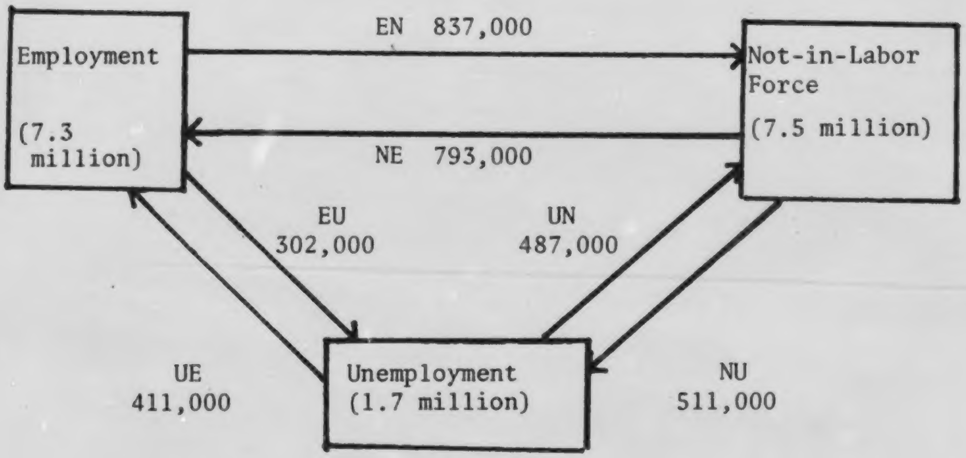
[2] We are grateful for the cooperation of Bureau of Labor Statistics staff in making these data available. More comprehensive descriptions of the data and their potential for research on labor market dynamics can be found in H. Hilaski, "The Status of Research on Gross Changes in the Labor Force," Employment and Earnings, October 1968.

Figure 1

Labor Market Stocks and Monthly Flows: 16-19 Year Olds
(1976 Monthly Averages)


Employment
(7.3 million)
EN 837,000
Not-in-Labor Force
(7.5 million)
NE 793,000
EU
302,000
UN
487,000
UE
411,000
Unemployment
(1.7 million)
NU
511,000

The volatility of the youth labor market is obvious from these statistics. For example, on average in 1976, approximately 900,000 teens or 53% of those unemployed one month either found a job or decided to leave the labor market the next month. The average monthly figures also show that approximately 1.3 million or 9% of the teenage labor force leaves the labor market each month, while another 1.3 million or 18% of those out of the labor market decide to enter.

For sixteen to nineteen-year olds, these large inter-labor market flows are dominated by transitions between school and work. Seasonal patterns in flow data provide a better idea of the vast numbers of people who change labor force states each month to generate the changes in stocks reported by the Current Population Survey. In June, when most teenagers are out of school and looking for summer employment, CPS statistics show the size of their labor force increasing by 1.9 million. The gross flow tabulations indicate that this change is generated by status changes of 3.7 million teenagers moving between states, with 2.8 million either becoming employed or unemployed when they are not in the labor force in the preceding month, and with almost 1 million deciding to drop out of the labor force for the summer. The data also indicate that, although the large increase in unemployment in June is primarily due to a dramatic increase into the labor force, it is aided by reductions in flows out of unemployment. In September, when school reopens, the CPS labor force declines by 2.1 million teenagers, which is caused by 3.1 million withdrawing from the labor force and 1 million entering it.

While some of these observations are intuitive, they do suggest the potential the data have for providing insights into trends and cyclical patterns of labor force participation; determinants of unemployment among various labor force groups; and information on the relative impact of discouragement and labor force withdrawal on different demographic groups.

## Errors in Data

Despite their potential for analyzing labor market dynamics, gross flow data have gone unpublished since 1952 and their use in research has been sporadic due to serious reservations concerning their reliability. Problems with the data are generally divided into two components: sampling variability and response variability.

Of the two possible sources of error, sampling variability is the less serious, and is the result of reducing the full CPS sample to only those individuals who are interviewed for two consecutive months. By the time gross flow data are tabulated, sample size is reduced to approximately 65% of the full CPS survey. The exclusion of nonidenticals, who either fail to respond, are absent from home during the survey week or who have moved from the household, make random error more prominent in gross flow than stock data. It should be noted, however, that if nonidenticals differ in characteristics from identical individuals, this reduction in sample size could cause the error to be systematic.[3] For example, teenagers who leave home by the time they are nineteen may be more prone to be in the labor force than those who remain with their families. Because they have moved from the interview unit, these youth will be underrepresented in the gross flow sample, and those remaining in the sample will tend to understate labor force attachment.

Response variability, which is the result of either misclassification of reported labor market status or "rotation group bias," is considered more important than sampling variability. Misclassification errors which tend to cancel when determining net changes in labor force stocks are a more serious problem with gross change data. In the past, the effect of misclassification has been assumed to be random with an overall effect of inflating the flows depicting movement between labor force states. It is hard to determine the magnitude or nature of the misclassification problem for teenagers. The fact that an adult usually reports labor force status for teenagers in a household and that it is often hard to determine the exact status of a teenager's labor force attachment could serve either to exaggerate movements between employment states or to suggest no change when, in fact, movements would be indicated if the youth had been directly surveyed.

The second cause of response variability is "rotation group bias"--a problem which generates systematic errors and which is related specifically to panel surveys. The hypothesis concerning this source of error is that the process of reinterview conditions individuals and patterns their response over time.[4] The eight rotation groups comprising the CPS, which differ only with respect to time in sample, consistently demonstrate different labor force characteristics. Census Bureau staff have been aware of the problem since the 1950s, but little is known about why it exists or the extent of its effect on the data either qualitatively or quantitatively. It is known, however, that groups newly entering the sample show higher levels of unemployment and marginal types of employment such as part-time work, unpaid family help, and domestics--those categories most likely to contain substantial numbers of people with weak labor force attachment. In subsequent months, a number of the individuals in these groups, who initially reported being employed or unemployed, report being out of the labor force. This type of change tends to exaggerate the size of the flows leaving the labor force and, therefore, understates gross change estimates of participation. Given the marginal attachment of teenagers to the labor market, it is possible that misclassification and rotation group bias problems contribute significantly to errors in measuring their changes in labor force status.

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[3] See Susan Palmer, Bureau of the Census, "On the Character and Influence of Nonresponse in the Current Population Survey," Proceedings of the Social Statistics Section, 1967, American Statistical Association, pp. 73-80.

[4] See Barbara A. Bailar, "The Effects of Rotation Group Bias on Estimates from Panel Studies," Journal of the American Statistical Association, vol. 70 (March 1975), pp. 23-30.

## Data Adjustment

Despite the sampling and response variability problems, gross flow data have been used by researchers to analyze labor market behavior. The data have produced intuitively acceptable results and have supplied a new dimension on market dynamics unavailable in other data bases. Earlier work with gross flow data, however, concentrated on cyclical variability of unemployment and did not concentrate on participation behavior which would suffer much more from the problems associated with rotation group bias.[5]

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5. See W. Lee Hansen. "The Cyclical Sensitivity of Labor Supply," American Economic Review, (June 1941), pp. 299-309; George L. Perry, "Unemployment Flows in the U.S. Labor Market," Brookings Paper on Economic Activity, vol. 2 (1972), pp. 245-78;Claire B. Vickery, "Why Unemployment Rates Differ by Race and Sex," unpublished Ph.D. dissertation, University of Maryland, 1973; Thomas F. Bradshaw, Bureau of Labor Statistics, "Employment in Perspective: A Cyclical Analysis of Gross Flows in the Labor Force," Report 508,

The bias problems in the data cannot be easily explained and no one has yet determined their magnitude or exact effect on the various transition flows. However, the fact that they exist can be easily ascertained by comparing net changes in labor force stocks derived from the CPS with those derived from gross flow data. If one were to accept the accuracy of the full CPS,[6] failure of the gross flow data to replicate these changes is an obvious warning that there are problems measuring the flows. In some instances, even the direction of the change is inconsistent. While a portion of the problem is attributable to random sampling error, the discrepancies do suggest that systematic errors are present and have to be accounted for when using the data.[7]

The procedure developed to adjust the gross flow data does not attempt to explain the causes of error; it merely attempts to force consistency with the data coming from the full CPS survey.[8] Using multivariate regression analysis, monthly changes in employment status measured by gross flow data are regressed against the same changes measured by the CPS. If earlier conjectures on the nature of biases in the data are correct, the results of the procedure would be

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5. (cont.) 1977; Richard S. Toikka, "A Markovian Model of Labor Market Decisions by Workers," The American Economic Review, vol. 66 (December 1976), pp. 821-34.

6. The rotation group bias problem affecting gross change data also affect the estimates of monthly levels of labor force statistics reported in the full CPS. If it can be assumed that bias patterns across the eight survey panels are consistent from one month to the next, the effects of the bias on the full sample will cancel in computing month to month changes. It is the "unbiased" month changes which are used in the adjustment procedure. Levels of labor force stocks in the full CPS are affected by rotation group bias. See Bailar, op. cit.

7. See Stuart H. Altman, "Factors Affecting the Unemployment of Married Women: A Study of the Dynamics Affecting the Labor Force Behavior of Secondary Family Workers," unpublished Ph.D. dissertation, University of California at Los Angeles, 1964.

8. Smith, op. cit.

a reduction of the flow variables which depict movements between employment, unemployment and non-labor force status all of which tend to be exaggerated by misclassification errors. The most pronounced reductions should be in the two flows out of the labor force, EN and UN, which are also inflated by rotation group bias.

Since the gross flow subsample does not take account of monthly increases in population which result from new population entrants or net immigration, the CPS changes used in the adjustment procedure were modified to exclude population growth. Adjusted changes in stocks from the CPS were constructed using the previous month's stock levels weighted by a ratio of current to preceding month's population. This assumes that entrants to the CPS sample demonstrate the same characteristics as those already in the population group. The changes in stocks measured by gross flows are computed by calculating the new inflow to those stocks. The relevant identities for each group become:

$$(1) \quad \Delta E_i = E_i - E_{i,-1} \cdot \left( \frac{P_i}{P_{i,-1}} \right) = UE_i + NE_i - EU_i - EN_i,$$

$$(2) \quad \Delta U_i = U_i - U_{i,-1} \cdot \left( \frac{P_i}{P_{i,-1}} \right) = EU_i + NU_i - UE_i - UN_i,$$

$$\text{and, } (3) \quad \Delta N_i = N_i - N_{i,-1} \cdot \left( \frac{P_i}{P_{i,-1}} \right) = EN_i + UN_i - NE_i - NU_i,$$

with the left side estimated from the full CPS and the right from gross flow data.

The aim of the procedure is to obtain a correction factor on each flow variable. As can be seen above, each flow appears in two identity relationships. We therefore pooled observations, and ran a single regression based on data from June 1967 through September 1977, simultaneously estimating changes in employment, unemployment and nonparticipation and producing one adjustment factor for each flow.

The procedure has gone through a number of modifications. Currently it produces cyclical correction factors which vary with aggregate labor market conditions, represented by the aggregate unemployment rate lagged one period. The resulting relationship between an adjusted flow, f', and an observed gross flow, f, thus becomes:

$$(4) \quad f' = (B_f + B_{If} \cdot (\tfrac{U}{L})_{-1})\, f,$$

where $B_f$ and $B_{If}$ are coefficients from a constrained identity regression.[9]

Potentially there is a problem applying the procedure to groups as small as the four teenage groups being analyzed in this paper. The smaller the population, the more likely it will be that random error will dominate the systematic error we are attempting to eliminate. As a consequence, among sixteen- to nineteen-year olds, nonwhites will suffer more than whites because of sample size. Among the flow variables, the flows between employment and unemployment are the most affected since youth behavior is dominated by their labor force withdrawal and reentry.

Table 1 reports average corrections applied to the six gross flow variables for the labor force groups analyzed in the next section. The adult white male group is used as a point of comparison. In four of the six flows for this group there is a substantial reduction in the reported statistics. The implied overstatement of measured data is consistent with the hypothesis that misclassification error exaggerates the number of people changing labor force states. The severe reduction in EN is consistent with hypotheses on rotation group bias. We have no explanation for the seeming undermeasurement of flows between employment and unemployment (EU) and between unemployment and not in the labor force (UN). This is especially true for UN since it should be influenced by the same rotation group bias problems affecting the employment to not in the labor force flow. We have less confidence in the parameter estimates for the correction on UN since relatively few white male adults exit the labor force from unemployment.

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[9] For a more complete description of the procedure see Charles C. Holt, et al., "Labor Markets, Inflation and Manpower Policies," Final Report to the U.S. Department of Labor, Appendix C, May 1975.

The correction technique generally yields smaller bias adjustments for teenagers than for white male adults. With the exception of the small nonwhite female cohort, which demonstrates the lowest measured inter-labor force mobility, most flows are adjusted to within 15% of their observed value. While this may result from more accurate data for this group, it is more likely the result of random measurement error due to small sample size of offsetting measurement problems unique to determining the labor force status of teenagers. The suggested reduction in the UN flow is again consistent with rotation group bias problems; the increase in the EN flow is not. However, as indicated earlier, no one truly understands the nature of biases in the data and the technique used for its adjustment only provides a statistical reconciliation of gross flow and CPS stock data. It does not provide any reasons for discrepancies.

TABLE 1
AVERAGE GROSS CHANGE CORRECTION FACTORS
July 1967 to September 1977[a]

| Group/Flow | EN | EU | UN | NU | UE | NE |
|---|---|---|---|---|---|---|
| 16-19 year olds: | | | | | | |
| White Males | 1.02 | 1.10 | 1.05 | 1.05 | .95 | 1.02 |
| White Females | 1.06 | .99 | .89 | 1.06 | .98 | .98 |
| Nonwhite Males | 1.00 | .91 | .97 | 1.03 | .89 | .98 |
| Nonwhite Females | 1.03 | .71 | .89 | 1.10 | 1.20 | .87 |
| 25-59 year olds: | | | | | | |
| White Males | .56 | 1.05 | 1.17 | .72 | .92 | .78 |

a. Based on unpublished estimates by the authors.

## TEENAGE LABOR MARKET PATTERNS: INSIGHTS FROM GROSS FLOW DATA

The remainder of this paper focusses on an examination of the labor market behavior of teenagers during the past decade. Several patterns observed with conventional labor force and unemployment series are examined more closely with estimates based on the adjusted gross flow data:

(1) The labor force participation rates of white teenagers, particularly females, have risen sharply, while those of black male teenagers fell and those of black female teenagers were fairly stable.

(2) Each teenage group's participation rate exhibits some cyclical variation. Between 1974 and 1975, as the aggregate unemployment rate rose by three percentage points, the secular increases in the participation rates of white male and female teens were reversed and the decline in black male participation was accelerated. The participation of black female teens did increase in this recession, although it fell three points in the milder 1971 recession.

(3) Unemployment rates of every teenage group were much higher than adult unemployment rates throughout the decade and were considerably worsened by the recession.

(4) The position of black teenagers relative to white teenagers, measured by their unemployment rates and their participation rates, has dramatically deteriorated over the decade. The recession widened the gap still further.

These and other patterns in the regular CPS series are also found in the gross flow data and, to some extent, are explained by them. Net increases in unemployment occur when the gross flows into unemployment exceed the flows out. Chronic differences in unemployment rates between two groups either reflect differences in their vulnerability to unemployment (the probability of becoming unemployed) or in their propensity to leave unemployment, or both. Similar reasoning can be used to account for changes or differences in participation patterns.

The equations reported in the remainder of this section are taken from our gross flow model of the labor market and depict determinants of the proportion of a group's population moving from one labor market state to another. All transitions affect the group's unemployment or labor force size directly or indirectly. The transition rates are modeled as group probabilities which are related to aggregate labor market conditions, a secular trend, and a set of seasonals. The means of these rates for the past decade are provided in the Appendix.

When analyzing regression estimates based on gross change data, it must be remembered that the data suffer much more from sample variability than the stock data reported in the full CPS. The size of the gross flow subsample is only about 67% of that of the CPS due to the requirements of matching individual responses for consecutive months. The relatively small populations of the race/sex teenage cohorts reported in this paper further exacerbates the problem. This problem increases the variance of our parameter estimates. However, for purposes of this paper, we are not so much interested in the numerical value of parameters as in the importance of aggregate labor market conditions, seasonals and time trends in explaining the labor force behavior of sixteen- to nineteen-year olds over the past decade. Negative or positive responses to explanatory variables and their level of significance can, in themselves, provide explanations for labor force participation behavior and cyclical response not available from the stock data.

### Transition Probability Equations

The analysis begins with the estimation of two sets of equations that depict the movement of people into the labor force. As illustrated by Figure 1, the number of teenagers who enter the labor force in a month is determined by the number in the potential entrant pool (i.e, not in the labor force) and their probability of entry. Some entrants are unemployed at the time of the first survey in which they are in the labor force; others have either entered with jobs or found them between survey periods. Both the decision to enter and the outcome of that decision may be affected by cyclical and seasonal factors and may change over time.

Table 2 and 3 report the equations used to estimate each group's monthly probability of labor force entry and the proportion of the entrants that have jobs ("successful labor force entry"). In each of these equations, as well as the ones that follow, the transition probability is expressed as a multiplicative function of the aggregate job vacancy-unemployment ratio lagged one month, an exponential time trend, and a set of seasonal dummies. Estimates are reported for each of the four race/sex teenage groups and, for comparison, for prime-age white males. The estimation period is July 1967 through September 1977.

In the labor force entry equation (Table 2), the dependent variable is the log of the proportion of the group's nonparticipants who entered the civilian labor force since the preceding month's survey period. The gross flow data used in the numerator were first corrected with the procedures described in the preceding section. The aggregate labor market variable is the log of the ratio of the Conference Board's Help-Wanted Index and the CPS aggregate unemployment series, both seasonally unadjusted and lagged by one month. The trend term T takes on a value of one in July, 1967 and is incremented by one each successive month. Eleven seasonal dummies, corresponding to January through November, each take the value of one or zero, with the coefficient in December constrained to equal the negative of the sum of the other coefficients. $\xi_{nli}$ is a random error term with zero mean and constant variance. The numbers in parenthesis below the parameter estimates are corresponding T-statistics. Each column also reports the regression coefficient of determination corrected for degrees of freedom, $\bar{R}^2$; the standard error of estimation, S.E.; and the Durbin-Watson Statistic, D.W. Significance at the .05 level and the .01 level are indicated by a single asterisk and a double asterisk respectively. The joint influence of the seasonals is measured by the F-statistic.

## TABLE 2

## PARAMETERS DETERMINING PROBABILITY OF LABOR FORCE ENTRY

$$\ln(NL_i/N_{i,-1}) = \ln k_{nli} + \beta_{nli}\ln(V/U)_{-1} + g_{nli}T + S1 + \ldots + S11 + \varepsilon_{nli}$$

| | (1) WHITE MALES 16-19 | (2) WHITE FEMALES 16-19 | (3) NONWHITE MALES 16-19 | (4) NONWHITE FEMALES 16-19 | (5) WHITE MALES 25-59 |
|---|---|---|---|---|---|
| $\ln k_{nli}$ | -1.498 | -2.003 | -1.358 | -2.222 | -1.715 |
| | (-14.69)** | (-22.05)** | (-8.10)** | (-11.37)** | (-15.21)** |
| $\beta_{nli}$ | 0.061 | 0.006 | 0.099 | -0.037 | 0.103 |
| | (1.97)* | (0.23) | (1.94) | (-0.62) | (3.01)** |
| $g_{nli}$ | 0.002 | 0.002 | -0.001 | -0.000 | -0.003 |
| | (5.68)** | (7.08)** | (-1.11) | (-0.58) | (-6.28)** |
| S1 | -0.272 | -0.294 | -0.236 | -0.237 | 0.020 |
| | (-8.64)** | (-10.35)** | (-4.55)** | (-3.92)** | (0.57) |
| S2 | -0.317 | -0.211 | -0.266 | -0.292 | -0.008 |
| | (-9.81)** | (-7.25)** | (-5.01)** | (-4.72)** | (-0.21) |
| S3 | -0.274 | -0.249 | -0.315 | -0.300 | -0.042 |
| | (-8.58)** | (-8.66)** | (-6.01)** | (-4.91)** | (-1.18) |
| S4 | -0.107 | -0.219 | -0.245 | -0.358 | -0.008 |
| | (-3.34)** | (-7.61)** | (-4.66)** | (-5.85)** | (-0.23) |
| S5 | -0.052 | -0.099 | -0.215 | -0.230 | 0.016 |
| | (-1.63) | (-3.40)** | (-4.06)** | (-3.71)** | (0.46) |
| S6 | 0.834 | 0.702 | 0.844 | 0.868 | 0.085 |
| | (25.52)** | (23.84)** | (15.70)** | (13.85)** | (2.35)* |
| S7 | 0.670 | 0.498 | 0.826 | 0.643 | -0.008 |
| | (21.75)** | (17.94)** | (16.33)** | (10.91)** | (-0.24) |
| S8 | 0.185 | 0.125 | 0.440 | 0.185 | 0.057 |
| | (6.02)** | (4.52)** | (8.71)** | (3.14)** | (1.67) |
| S9 | -0.033 | 0.070 | -0.135 | 0.038 | 0.113 |
| | (-1.07) | (2.53)* | (-2.66)** | (0.64) | (3.31)** |
| S10 | -0.115 | -0.012 | -0.082 | 0.016 | -0.013 |
| | (-3.56)** | (-0.41) | (-1.56) | (0.26) | (-0.37) |
| S11 | -0.275 | -0.162 | -0.259 | -0.120 | -0.076 |
| | (-8.57)** | (-5.60)** | (-4.91)** | (-1.96) | (-2.13)* |
| F(11, 109) Seasonals | 135.42** | 109.93** | 68.61** | 38.18 | 3.32** |
| $\bar{R}^2$ | 0.928 | 0.917 | 0.864 | 0.772 | 0.604 |
| S.E. | 0.106 | 0.095 | 0.174 | 0.203 | 0.117 |
| D.W. | 1.793 | 2.068 | 1.988 | 1.758 | 1.533 |

The first column shows our estimates of the determinants of the monthly probability of a white male teenager entering the labor force. Over the ten-year estimation period, approximately 20% of nonparticipants in this group entered the labor force each month. The estimates in column 1 indicate that: (1) the probability of entering in any particular month is strongly associated with the state of the business cycle; (2) this probability has been increasing over time; and (3) it has a strong seasonal component. June and July, of course, are the months when a teenager is most likely to enter the labor force. About 92% of the monthly variation in the probability of entering the labor force for this group is explained by our equation. Most of this variation is explained by the seasonal dummies.

White female teenagers show no significant cyclical sensitivity in their entry patterns, but do show a strong, positive entry trend; nonwhite males have a procyclical entry pattern, but no trend; and nonwhite female entry rates have no significant trend or cyclical patterns. By way of comparison, prime-age white males, whose coefficients are shown in the last column, have a positive cyclical entry pattern, a negative entry trend, and a seasonal pattern which, although significant, is much smaller than that of any of the teenage groups.

Our strongest results concern the probability that a teenager who enters the labor force will have a job during the month that he or she enters (Table 3). For the decade as a whole, white teenagers were much more likely than nonwhite teenagers to be successful entrants. About 66% of white males and 63% of white females had jobs in the entry month compared with only 47% on nonwhite males and 33% of nonwhite females.

As should be expected, for every group the probability of a new entrant or re-entrant having a job varied procyclically (i.e., a positive coefficient on the vacancy-unemployment term). Successful entry was closely related to the state of the aggregate labor market. A startling finding was the significant negative trends found for

## TABLE 3

## PARAMETERS DETERMINING PROBABILITY OF SUCCESSFUL LABOR FORCE ENTRY

$$\ln(NE_i/NL_i) = \ln k_{nei} + \beta_{nei}\ln(V/U)_{-1} + g_{nei}T + S1 + \ldots + S11 + \varepsilon_{nei}$$

| | (1) WHITE MALES 16-19 | (2) WHITE FEMALES 16-19 | (3) NONWHITE MALES 16-19 | (4) NONWHITE FEMALES 16-19 | (5) WHITE MALES 25-59 |
|---|---|---|---|---|---|
| $\ln k_{nei}$ | 0.260<br>(4.84)** | -0.147<br>(-2.56)* | 0.050<br>(0.27) | 0.954<br>(2.58)* | 0.327<br>(4.65)** |
| $\beta_{nei}$ | 0.162<br>(9.98)** | 0.056<br>(3.23)** | 0.190<br>(3.38)** | 0.473<br>(4.13)** | 0.173<br>(8.16)** |
| $g_{nei}$ | -0.001<br>(-4.23)** | -0.001<br>(-6.66)** | -0.001<br>(-1.63) | -0.004<br>(-3.02)** | -0.001<br>(-3.61)** |
| S1 | -0.000<br>(-0.02) | -0.005<br>(-0.26) | -0.024<br>(-0.41) | -0.090<br>(0.79) | -0.024<br>(-1.09) |
| S2 | -0.095<br>(-5.60)** | -0.006<br>(-0.31) | -0.099<br>(-1.69) | -0.044<br>(-0.37) | -0.075<br>(-3.39)** |
| S3 | -0.071<br>(-4.24)** | -0.015<br>(-0.82) | 0.009<br>(0.16) | -0.394<br>(-3.40)** | -0.050<br>(-2.28)* |
| S4 | 0.050<br>(2.96)** | 0.012<br>(0.66) | 0.019<br>(0.33) | 0.039<br>(0.34) | 0.021<br>(0.96) |
| S5 | 0.067<br>(3.94)** | 0.006<br>(0.33) | 0.144<br>(2.46)* | -0.111<br>(-0.95) | 0.026<br>(1.16) |
| S6 | 0.014<br>(0.80) | -0.067<br>(-3.64)** | -0.158<br>(-2.65)** | -0.147<br>(-1.23) | -0.019<br>(-0.86) |
| S7 | 0.051<br>(3.20)** | 0.046<br>(2.67)** | 0.182<br>(3.25)** | 0.278<br>(2.49)* | 0.038<br>(1.83) |
| S8 | 0.077<br>(4.76)** | 0.001<br>(0.10) | 0.092<br>(1.65) | 0.204<br>(1.83)* | 0.008<br>(0.40) |
| S9 | -0.028<br>(-1.72) | 0.004<br>(-0.20) | -0.025<br>(-0.45) | -0.146<br>(-1.30) | 0.077<br>(3.62)** |
| S10 | 0.35<br>(2.07)* | 0.024<br>(1.34) | 0.001<br>(0.01) | 0.107<br>(0.92) | 0.022<br>(0.983) |
| S11 | -0.042<br>(-2.49)* | -0.04<br>(-2.34)* | -0.133<br>(-2.28)* | -0.030<br>(-0.25) | -0.029<br>(-1.33) |
| F(11,109) Seasonals | 11.05** | 2.95** | 2.94** | 2.39** | 3.33** |
| $\bar{R}^2$ | 0.812 | 0.626 | 0.351 | 0.472 | 0.727 |
| S.E. | 0.056 | 0.060 | 0.192 | 0.383 | 0.073 |
| D.W. | 1.802 | 2.036 | 2.304 | 2.064 | 2.326 |

every group. That is, adjusting for cyclical and seasonal fluctuations, it is becoming increasingly difficult for an entrant or re-entrant to immediately find a job, with the steepest decline estimated for nonwhite females. This negative trend, incidentally, was found for prime-age white males as well.

Once an individual is in the labor force, the probability of leaving in a given month may be related to cyclical, trend, and seasonal factors. We distinguish between the probability of leaving the labor force if employed and the probability of leaving the labor force if unemployed. The next two tables report our parameter estimates for each category. No uniform cyclical pattern in exit rates was found. Looking first at the probability of leaving the labor force if employed, Table 4 shows a significant reduction for white females in the probability of leaving as job opportunities expand, but an increase for the two nonwhite teenage groups and for prime-age males. Negative trends were estimated for both white teenage groups and for nonwhite females. There is an obvious seasonal pattern, with the probability of a teenager leaving the labor force extremely high in September.

Table 5 shows that the probability of leaving the labor force if unemployed is positively related to the state of the aggregate labor market for white teenagers and negatively related for nonwhite female teens. A negative trend in the exit rate is estimated for the white teenage groups and a positive trend is estimated for nonwhite females. The probability of leaving is much lower than average in the months of June and July.

The mixed findings with respect to cyclical patterns in the probabilities of teenagers leaving the labor force from employment and unemployment raise the question of the cause of the cyclical labor force exit patterns, commonly associated with labor force discouragement. We offer a very simple explanation, supported by the gross change statistics: unemployed people regardless of age, race, sex, or state of the ecomony, are more likely to leave the labor force than

## TABLE 4

## PARAMETERS DETERMINING PROBABILITY OF LABOR FORCE EXIT FROM EMPLOYMENT

$$\ln(EN_i/E_{i,-1}) = \ln k_{eni} + \beta_{eni}\ln(V/U)_{-1} + g_{eni}T + S1 + \ldots + S11 + \varepsilon_{eni}$$

| | (1) WHITE MALES 16-19 | (2) WHITE FEMALES 16-19 | (3) NONWHITE MALES 16-19 | (4) NONWHITE FEMALES 16-19 | (5) WHITE MALES 25-59 |
|---|---|---|---|---|---|
| $\ln k_{eni}$ | -2.102<br>(-17.82)** | -2.117<br>(-21.75)** | -1.393<br>(-5.08)** | -1.122<br>(-4.25)** | -5.226<br>(-46.87)** |
| $\beta_{eni}$ | 0.008<br>(0.23) | -0.075<br>(-2.56)** | 0.184<br>(2.22)* | 0.142<br>(1.77) | 0.156<br>(4.62)** |
| $g_{eni}$ | -0.002<br>(-5.18)** | 02.003<br>(-9.23)** | 0.001<br>(1.00) | -0.002<br>(-2.35)* | 0.002<br>(3.83)** |
| S1 | 0.064<br>(1.75) | 0.163<br>(5.41)** | 0.116<br>(1.37) | 0.046<br>(0.56) | 0.160<br>(4.64)** |
| S2 | -0.127<br>(-3.41)** | -0.151<br>(-4.90)** | -0.175<br>(-2.01)* | -0.048<br>(-0.57) | -0.186<br>(-5.27)** |
| S3 | -0.220<br>(-5.96)** | -0.145<br>(-4.76)** | -0.349<br>(-4.06)** | -0.370<br>(-4.48)** | -0.103<br>(-2.94)** |
| S4 | -0.157<br>(-4.24)** | -0.038<br>(-1.24) | -0.176<br>(-2.05)* | -0.090<br>(-1.09) | -0.001<br>(-0.04) |
| S5 | -0.018<br>(-0.49) | -0.003<br>(-0.09) | 0.027<br>(0.31) | -0.134<br>(-1.61) | 0.040<br>(1.13) |
| S6 | -0.325<br>(-8.60)** | -0.065<br>(-2.10)* | -0.280<br>(-3.20)** | -0.024<br>(-0.29) | -0.083<br>(-2.32)* |
| S7 | -0.473<br>(-13.30)** | -0.261<br>(-8.89)** | -0.355<br>(-4.29)** | -0.073<br>(-0.92) | 0.118<br>(3.51)** |
| S8 | 0.163<br>(4.59)** | 0.092<br>(3.12)** | 0.213<br>(2.58)** | 0.299<br>(3.77)** | 0.064<br>(1.88) |
| S9 | 1.038<br>(29.09)** | 0.852<br>(28.91)** | 1.125<br>(13.58)** | 1.001<br>(12.54)** | 0.076<br>(2.24)* |
| S10 | 0.163<br>(4.37)** | 0.039<br>(1.26) | -0.050<br>(-0.57) | -0.050<br>(-0.61) | -0.039<br>(-1.12) |
| S11 | 0.010<br>(0.28) | -0.164<br>(-5.37)** | 0.009<br>(0.11) | -0.244<br>(-2.95)** | 0.016<br>(0.45) |
| F(11,109) Seasonals | 101.98** | 94.04** | 20.87** | 18.44** | 7.48** |
| $\bar{R}^2$ | 0.906 | 0.902 | 0.655 | 0.648 | 0.414 |
| S.E. | 0.122 | 0.101 | 0.284 | 0.273 | 0.116 |
| D.W. | 1.804 | 1.898 | 1.872 | 2.300 | 2.155 |

## TABLE 5

## PARAMETERS DETERMINING PROBABILITY OF LABOR FORCE EXIT FROM UNEMPLOYMENT

$$\ln(UN_i/U_{i,-1}) = \ln k_{uni} + \beta_{uni}\ln(V/U)_{-1} + g_{uni}T + S1 + \ldots + S11 + \varepsilon_{uni}$$

| | (1) WHITE MALES 16-19 | (2) WHITE FEMALES 16-19 | (3) NONWHITE MALES 16-19 | (4) NONWHITE FEMALES 16-19 | (5) WHITE MALES 25-59 |
|---|---|---|---|---|---|
| $\ln k_{uni}$ | -0.349<br>(-2.90)** | -0.622<br>(-5.24)** | -1.169<br>(-5.43)** | -1.600<br>(-7.84)** | 0.254<br>(1.18) |
| $\beta_{uni}$ | 0.178<br>(4.87)** | 0.084<br>(2.33)* | -0.010<br>(-0.15) | -0.121<br>(-1.96) | 0.669<br>(10.57)** |
| $g_{uni}$ | -0.003<br>(-6.57)** | -0.003<br>(-7.22)** | 0.000<br>(0.47) | 0.002<br>(2.35)* | -0.000<br>(-0.13) |
| S1 | 0.187<br>(5.07)** | 0.226<br>(6.18)** | 0.220<br>(3.31)** | 0.031<br>(0.50) | 0.315<br>(4.73)** |
| S2 | -0.050<br>(-1.30) | -0.006<br>(-0.16) | -0.090<br>(-1.32) | 0.053<br>(0.82) | -0.111<br>(-1.63) |
| S3 | 0.037<br>(0.99) | 0.008<br>(0.22) | 0.038<br>(0.57) | -0.040<br>(-0.62) | -0.183<br>(-2.71)** |
| S4 | 0.149<br>(3.94)** | 0.140<br>(3.76)** | 0.074<br>(1.09) | 0.062<br>(0.97) | -0.080<br>(-1.19) |
| S5 | 0.186<br>(4.90)** | 0.117<br>(3.14)** | 0.182<br>(2.68)** | 0.153<br>(2.37)* | -0.098<br>(-1.44) |
| S6 | -0.284<br>(-7.36)** | -0.141<br>(-3.72)** | -0.153<br>(-2.23)* | -0.196<br>(-2.99)** | -0.166<br>(-2.41)* |
| S7 | -0.289<br>(-7.95)** | -0.096<br>(-2.69)** | -0.236<br>(-3.63)** | -0.181<br>(-2.95)** | -0.033<br>(0.51) |
| S8 | 0.057<br>(1.57) | 0.027<br>(0.81) | -0.112<br>(-1.72) | 0.084<br>(1.36) | 0.076<br>(1.17) |
| S9 | 0.150<br>(4.17)** | -0.051<br>(-1.41) | 0.322<br>(4.94)** | 0.104<br>(1.69) | -0.069<br>(1.06) |
| S10 | -0.083<br>(-2.19)* | -0.160<br>(-4.26)** | -0.097<br>(-1.42) | -0.046<br>(-0.72) | 0.063<br>(1.93) |
| S11 | -0.027<br>(-0.72) | -0.065<br>(-1.75) | -0.056<br>(-0.82) | -0.027<br>(-0.42) | 0.096<br>(1.42) |
| F(11,109) Seasonals | 17.97** | 8.97** | 6.04** | 2.63** | 4.18** |
| $\bar{R}^2$ | 0.767 | 0.658 | 0.312 | 0.306 | 0.688 |
| S.E. | 0.125 | 0.123 | 0.223 | 0.212 | 0.223 |
| D.W. | 1.594 | 1.706 | 1.630 | 1.800 | 1.557 |

employed people in the same demographic group. Hence, an increase in the unemployment rate generates a higher labor force exit rate merely by shifting more people into the exit-prone unemployment stock even if the probability of exit from that stock does not change.[10]

Our last two sets of equations analyze the probabilities of transition between unemployment and employment. Table 6 provides our estimates of the relationship between the probability of unemployed persons finding a job each month and the same cyclical, trend and seasonal terms used in the preceding equations. For most teenage groups the probability of finding a job varies procyclically, as expected. For nonwhite females, we have an unexpected negative relationship. There is also a slight negative trend in the probability of an unemployed nonwhite male teenager finding a job. Seasonality plays a significant role for every group, with success highest in July and lowest in January.

Table 7 provides estimates of the relationship explaining the probability of an employed person becoming unemployed each month. This turnover probability is highest for nonwhite male teenagers and lowest for white female teens. We expected an inverse cyclical relationship for every group, but found it only for white female teens. The probability for most groups is highest in the months of January, June, and September and lowest in the spring.

## Other Factors

We recognize that the transition rates of teenagers are influenced by many variables that are not included in our regression equations.

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[10] For a steady state analysis of this phenomenon, based on previous transition probability estimates, see Ralph E. Smith, "The Discouraged Worker in a Full Employment Economy," Proceedings of the American Statistical Association, Business and Economics Section, 1974, pp. 210-25. Subsequent simulations confirmed its importance in a dynamic context.

## TABLE 6

## PARAMETERS DETERMINING PROBABILITY OF TRANSITION FROM UNEMPLOYMENT TO EMPLOYMENT

$$\ln(UE_{i,-1}/U_{i,-1}) = \ln k_{uei} + \beta_{uei}\ln(V/U)_{-1} + g_{uei}T + S1 + \ldots + S11 + \varepsilon_{uei}$$

| | (1) WHITE MALES 16-19 | (2) WHITE FEMALES 16-19 | (3) NONWHITE MALES 16-19 | (4) NONWHITE FEMALES 16-19 | (5) WHITE MALES 25-59 |
|---|---|---|---|---|---|
| $\ln k_{uei}$ | -0.333<br>(-2.90)** | -1.023<br>(-6.76)** | -0.234<br>(-0.77) | -2.610<br>(-7.34)** | -0.663<br>(-6.83)** |
| $\beta_{uei}$ | 0.244<br>(7.01)** | 0.085<br>(1.84) | 0.351<br>(3.79)** | -0.216<br>(-2.01)* | 0.102<br>(3.48)** |
| $g_{uei}$ | 0.001<br>(1.32) | 0.001<br>(1.59) | -0.002<br>(-1.96) | -0.000<br>(-0.27) | -0.002<br>(-4.29)** |
| S1 | -0.208<br>(-5.87)** | -0.388<br>(-8.29)** | -0.164<br>(-1.73) | -0.394<br>(-3.58)** | -0.203<br>(-6.77)** |
| S2 | -0.113<br>(-3.11)** | -0.047<br>(-0.98) | -0.095<br>(-0.99) | -0.099<br>(-0.88) | -0.065<br>(-2.10)* |
| S3 | -0.126<br>(-3.50)** | -0.105<br>(2.22)* | -0.035<br>(-0.37) | 0.105<br>(0.945) | -0.016<br>(-0.54) |
| S4 | 0.051<br>(1.43) | -0.109<br>(-2.30)* | -0.144<br>(-1.50) | -0.299<br>(-2.69)** | 0.075<br>(2.47)** |
| S5 | -0.029<br>(-0.78) | -0.039<br>(-0.82) | -0.170<br>(-1.76) | -0.066<br>(-0.59) | 0.089<br>(2.90)** |
| S6 | 0.152<br>(4.13)** | 0.067<br>(1.38) | 0.021<br>(0.21) | -0.016<br>(-0.14) | -0.046<br>(-1.46) |
| S7 | 0.356<br>(10.28)** | 0.279<br>(6.10)** | 0.540<br>(5.86)** | 0.552<br>(5.15)** | 0.027<br>(0.92) |
| S8 | 0.127<br>(3.68)** | 0.055<br>(1.22) | 0.291<br>(3.16)** | 0.191<br>(1.79) | 0.161<br>(5.50)** |
| S9 | 0.023<br>(0.67) | 0.108<br>(2.36)* | -0.176<br>(-1.89) | 0.101<br>(0.94) | 0.154<br>(5.25)** |
| S10 | 0.065<br>(1.80) | 0.140<br>(2.92)** | 0.124<br>(1.29) | 0.353<br>(3.14)** | 0.027<br>(0.88) |
| S11 | -0.101<br>(-2.80)** | -0.002<br>(-0.04) | -0.023<br>(-0.24) | -0.239<br>(-2.14)* | -0.035<br>(-1.15) |
| F(11, 109) Seasonals | 21.93** | 11.72** | 5.45** | 6.36** | 15.45** |
| $\bar{R}^2$ | 0.747 | 0.535 | 0.485 | 0.332 | 0.716 |
| S.E. | 0.119 | 0.157 | 0.317 | 0.369 | 0.101 |
| D.W. | 1.909 | 2.034 | 1.624 | 2.249 | 2.256 |

## TABLE 7

## PARAMETERS DETERMINING PROBABILITY OF TRANSITION FROM EMPLOYMENT TO UNEMPLOYMENT

$$\ln(EU_i/E_{i,-1}) = \ln k_{eui} + \beta_{eui}\ln(V/U)_{-1} + g_{eui}T + S1 + \ldots + S11 + \varepsilon_{eui}$$

| | (1) WHITE MALES 16-19 | (2) WHITE FEMALES 16-19 | (3) NONWHITE MALES 16-19 | (4) NONWHITE FEMALES 16-19 | (5) WHITE MALES 25-59 |
|---|---|---|---|---|---|
| $\ln k_{eui}$ | -3.235<br>(-20.37)** | -4.430<br>(-20.92)** | -3.057<br>(-8.38)** | -2.613<br>(-5.68)** | -6.554<br>(-46.84)** |
| $\beta_{eui}$ | 0.006<br>(0.13) | -0.190<br>(-2.96)** | -0.018<br>(-0.16) | -0.170<br>(1.22) | -0.483<br>(-11.53)** |
| $g_{eui}$ | 0.000<br>(0.59) | 0.002<br>(2.01)* | 0.002<br>(1.40) | -0.002<br>(-0.88) | 0.001<br>(1.19) |
| S1 | 0.252<br>(5.13)** | 0.152<br>(2.32)* | 0.224<br>(1.99)* | 0.178<br>(1.25) | 0.288<br>(6.67)** |
| S2 | -0.097<br>(-1.93) | -0.080<br>(-1.20) | -0.271<br>(-2.35)* | -0.269<br>(-1.85) | 0.144<br>(3.25)** |
| S3 | -0.211<br>(-4.24)** | -0.017<br>(-0.26) | -0.291<br>(-2.55)* | -0.112<br>(-0.78) | 0.063<br>(1.44) |
| S4 | -0.236<br>(-4.75)** | -0.231<br>(-3.48)** | -0.186<br>(-1.63) | -0.369<br>(-2.56)* | -0.072<br>(-1.64) |
| S5 | -0.233<br>(-4.65)** | -0.169<br>(-2.53)* | 0.025<br>(0.22) | -0.387<br>(-2.66)** | -0.128<br>(-2.91)** |
| S6 | 0.264<br>(5.19)** | 0.414<br>(6.10)** | 0.394<br>(3.37)** | 0.380<br>(2.57)* | -0.060<br>(-1.33) |
| S7 | 0.088<br>(1.83) | 0.048<br>(0.75) | 0.256<br>(2.33)* | 0.062<br>(0.44) | -0.090<br>(-2.12)* |
| S8 | -0.117<br>(-2.46)* | -0.162<br>(-2.53)* | -0.147<br>(-1.34) | 0.007<br>(0.05) | 0.080<br>(-1.91) |
| S9 | 0.171<br>(3.55)** | 0.289<br>(4.51)** | 0.236<br>(2.14)* | 0.440<br>(3.16)** | -0.088<br>(-2.08)* |
| S10 | -0.005<br>(-0.10) | 0.025<br>(0.37) | 0.124<br>(1.08) | 0.119<br>(0.82) | -0.041<br>(-0.93) |
| S11 | 0.201<br>(4.03)** | 0.063<br>(0.94) | -0.077<br>(-0.67) | 0.129<br>(0.89) | 0.072<br>(1.65) |
| F(11, 109) Seasonals | 13.58** | 9.15** | 3.97** | 3.20** | 7.36** |
| $\bar{R}^2$ | 0.530 | 0.521 | 0.234 | 0.212 | 0.812 |
| S.E. | 0.165 | 0.220 | 0.378 | 0.477 | 0.145 |
| D.W. | 1.853 | 2.218 | 1.763 | 1.822 | 1.410 |

In particular, decisions to enter or leave the civilian labor market and the outcomes of these decisions are closely related to decisions regarding schooling, marriage, fertility, and military service and to government activities such as minimum wages and public service employment. A comprehensive gross flow model of the teenage labor market should take these factors into account.

During the past decade, probably the most critical influence on teenage males was the Vietnam War. To explore the impact of the military buildup in the late-sixties, we re-estimated our equations with a dummy variable that took the value of one in 1967, 1968 and 1969, and zero in other years. Since the Vietnam years were also the period of lowest unemployment and were at the beginning of our estimation period, we were concerned that our cyclical and trend coefficients would be quite sensitive to the change in specification. A superior procedure, time and data permitting, would be to model directly the gross flows between the armed forces and each of the labor market stocks.

Our results confirmed the importance of Vietnam to the teenage labor market, but suggested that our trend and cycle estimates were not particularly sensitive. Every group's probability of finding a job when unemployed was increased during the Vietnam period (with significant Vietnam dummies for three of the groups and near-significance for white male teens). Also, the probability of a white male teen leaving the civilian labor force increased, while their probability of transition from employment to unemployment declined. Only two other Vietnam dummies were significant.[11] Among the original cyclical and trend terms, only one coefficient (a negative cyclical term on the white female exit rate from employment) switched signs and remained significant; only six of the twenty-five significant trend or cyclical coefficients lost significance; and two new trends were introduced (positive hire probabilities for white males and white females).

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[11] These were a positive dummy in the equation explaining the entry rate of nonwhite females and an unexpected negative dummy in the equation for the white female entry success rate.

### Interpretation of the Labor Force and Unemployment Patterns

What have we learned about the teenage participation and unemployment rates of the past decade by analyzing the underlying gross flows? First, there are strong trends in both the labor force entry rates and the exit rates that are underlying the growth in white teenage participation, with steeper trends for the females. Unless these trends change, the difference in particiaption rates between white male and white female teens will continue to narrow. These entry and exit trends were not present for nonwhites.

Second, the cyclical participation patterns of teenagers are generated, in part, by the positive link between their entry decisions and job availability. In addition, unemployed teenagers are much more likely than those with jobs to drop out of the labor force. This discouraged worker pattern serves to dampen the cyclical amplitude of their reported unemployment rates since only active jobseekers are counted. Since the recent recession reduced teenage participation, above-trend growth in their participation rates should be anticipated during the recovery and has begun.

Third, the chronically high unemployment rates of teenagers can be directly linked to their large flows through the labor market.[12]

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[12] An excellent analysis of differences in steady state unemployment rates between demographic groups is in Stephen T. Marston, "Employment Instability and High Unemployment Rates," Brookings Paper on Economic Activity vol. 1 (1976), pp. 169-203. Using our corrected transition probabilities for 1967 through 1973, Marston finds that the major factor accounting for the difference between teen and adult unemployment rates is the larger probability for teens of leaving employment. This is consistent with our findings.

One-third of white teenagers and over half of nonwhite teenagers experience unemployment during the month that they enter the labor force. In addition, over 3% of teens with jobs become unemployed each month, triple the rate for prime-age adults. Once unemployed, white teenagers' probability of finding a job is similar to that of adults, but the likelihood of leaving the labor force is much higher. Nonwhite teenagers have the additional problem of having much lower probabilities of finding a job in the month that they enter the labor force and much lower monthly probability of finding a job once unemployed. Also, if employed, their probability of becoming unemployed each month is much higher.

Fourth, the deterioration in the relative position of nonwhite teenagers can be traced to both secular and cyclical differences in their behavior. Not only are their rates of labor force entry failing to keep pace with those for white teenagers, but also their success in finding work is decreasing more rapidly, which discourages further participation. The widening gap between nonwhite and white teenage unemployment is contributing to the widening gap in their participation.

## CONCLUSIONS

One of the motivations for this paper was to call attention to a potentially valuable source of data on labor market dynamics of youth. Clearly, the data need to be improved before they can be considered as valid indicators of month-to-month changes in the state of the economy.[13] But their failure to provide monthly signals that always coincide with those provided by conventional labor market series does not eliminate their value as an analytic tool. The trend, cyclical, and seasonal patterns reported in this paper do conform to those of the conventional CPS labor force and unemployment series and help to explain them. Much of the monthly variation in the transition probabilities is systematic and accounted for by our equations. Of the twenty-four equations estimated for teenagers, fourteen had cyclical variation significant at the .05 level; fourteen had significant trends; and all had significant seasonality; for white teenagers, between 52% and 92% of the variation in the transition probabilities were explained by our regressions; for nonwhite teenagers, between 23% and 86% were explained.

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[13] For recommendations regarding the improvement and dissemination of gross flow tabulations see our paper, "Gross Change Data: The Neglected Data Base," to be published by the National Commission on Employment and Unemployment Statistics later this year.

In this paper we have not focussed on the policy implications of our estimates. One use of the estimates would be to test the potential effects on teenage employment and unemployment of alternative policy interventions. Typically, a program would have its direct impact on one or more transition probabilities which, in turn, would effect both employment and unemployment. For example, expanding the number of public service employment slots available to teenagers would increase their probability of being hired and would attract more teenagers into the labor force. In evaluating the outcome of a job-creation program, our estimates suggest the importance of measuring the labor force response as well as the impact on unemployment.

Earlier estimates of the transition probability equations have been used to assess the impact of macroeconomic policy on the teenage labor market.[14] It was found that the 1974-1975 recession had a severe impact on teenage employment which was only partly reflected in their unemployment rates. We estimated that macroeconomic policies alone could alleviate much of the problem, but would leave teenage unemployment and labor force discouragement rates substantially higher than those of adults. Structural remedies oriented toward the special labor market problems and seasonality of the youth labor market are needed.

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[14] Ralph E. Smith, "The Teenage Unemployment Problem--How Much Will Macro Policies Matter?" in The Teenage Unemployment Problem: What Are the Options? Report of a Congressional Budget Office Conference (Washington, D.C.: Government Printing Office), pp. 7-17.

The increasing difficulty that all teen groups are having in finding jobs must be diagnosed and cured. Teenagers whose families are not plugged into informal job referral networks need alternatives. This is especially the case for minorities. Much of the difference between white and nonwhite teenagers in their unemployment rates can be traced to the greater difficulty that nonwhites have in obtaining a job once in the market. In addition, nonwhite teenage job-seekers are more likely to become discouraged and withdraw from the labor force. Hence, the real difference in the labor market performance between the two groups is even greater than the difference in their unemployment rates. Ways must be found to eliminate these disparities. Our analysis of the past decade provides no basis whatsoever for optimism that the problems are being resolved.

## APPENDIX

### AVERAGE MONTHLY TRANSITION PROBABILITIES[a]
### (JULY 1967 - JUNE 1977)

| | White Male Teen | White Female Teen | Black Male Teen | Black Female Teen | White Male Age 25-59 |
|---|---|---|---|---|---|
| Labor Force Entry ($NL'_i/N_{i,-1}$) | .22 | .16 | .19 | .13 | .10 |
| Successful Entry ($NE'_i/NL'_i$) | .67 | .64 | .49 | .36 | .68 |
| Labor Force Exit From Employment ($EN'_i/E_{i,-1}$) | .11 | .14 | .15 | .18 | .003 |
| Labor Force Exit From Unemployment ($UN'_{,}/U_{i,-1}$) | .31 | .33 | .34 | .37 | .10 |
| Unemployment to Employment ($UE'_{,}/U_{i,-1}$) | .30 | .28 | .20 | .18 | .32 |
| Employment to Unemployment ($EU'_i/E_{i,-1}$) | .04 | .03 | .06 | .04 | .01 |

a. These are the mean proportions of the populations who make the indicated transitions. The flows on which these estimates are based were corrected, using the procedure discussed in the text.

# THE TRANSITION FROM SCHOOL TO WORK
# WITH JOB SEARCH IMPLICATIONS

By: Stanley P. Stephenson, Jr.*

## ABSTRACT

This study adapts a human capital model of schooling and earnings to focus on the transition period. The adaptation consists of two steps. First, unemployment incidence and duration after last leaving school, but prior to the first job taken, is included as an intervening part of the transition process. A second feature is the consideration of the extent to which job holding while in school alters subsequent unemployment and wage rates.

Data used in the analysis is the NLS panel of white and black young men who were fourteen- to twenty-four-years-old in 1966. Persons included in the sample were drawn from the period 1966 to 1971 and had stopped attending school during this period. White and black men were studied separately. The complete model comprised a recursive system of a schooling equation, an unemployment equation, and an hourly wage equation.

The main results concern the unemployment and wage equations. Holding a job while in school lowers the incidence and duration of later unemployment and raises the subsequent hourly wage for both white and black youth. Full-time job effects exceed part-time job effects in both equations, and all effects are highly significant statistically. Job search theory suggests some types of unemployment behavior may lead to a higher wage, and a positive but nonsignificant effect of unemployment on the post-school wage was found for black youth. For white youth, however, significant and negative impacts of unemployment on the post-school wage were found.

Policy implications of the analysis suggest that school-work programs may ease transition problems for some youth in terms of raising post-school wage rates and lowering youth unemployment in the time immediately after school. An important qualification is that the present study did not evaluate government-created jobs for youth which may differ from the in-school jobs held by youth in this study.

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*I am grateful for the research assistance of David Smull. The opinions are mine alone, however.

## INTRODUCTION

This paper examines the determinants of successful school to work transition among a nationally representative sample of black and white male youth. The analysis involves estimating the parameters in a standard recursive model of schooling and wage rates and considers two issues which have not been fully explored in previous empirical analyses. First, the transition year may involve an intervening period of unemployment during which the school leaver searches for a job. This search behavior is included in the recursive model as an added equation. Some consider such unemployment as an indicator of an unsuccessful transition. This view is questioned by some results obtained here. Secondly, the nature of the search process, as well as the overall ease of the transition process, may be altered by the youth's labor force status during his last year in school. Indeed, Freeman and Coleman in a recent report urged that federal youth policy should consider programs which include work experiences while in school as a means of easing school to work transition problems.[1] While no special programs are examined here, we can examine the impact of labor status in school on the subsequent job search process and on the wage rate on the job held after last leaving school.

## CONCEPTUAL ISSUES

### Defining the Process

How one specifies exactly when the school to work transition occurs is crucial to the interpretation of the labor market results obtained.[2]

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1. Richard Freeman and James Coleman, discussion in The Teenage Unemployment Problem: What are the Options?, Congressional Budget Office Report. (Washington: Government Printing Office, 1976).

2. Michael D. Ornstein, Entry into the American Labor Force (New York: Academic Press, 1976).

Perhaps the first job held is the point at which the transition process should be measured. Yet such a job, if it can be recalled, may have little relation to a youth's future career or adult labor market experience, and, as such, may not indicate a successful transition. A next step is to consider the job taken after leaving school. Yet, a number of analysts have discussed the difficulties arising from ambiguities in responses obtained in surveys which seek to identify the first full-time job after an individual left school.[3] Several solutions have been suggested for determining the first such job: (a) "the first job after you first attended regular school",[4] (b) "the first job at which the respondent worked for two or more consecutive weeks after discontinuing regular school",[5] and (c) when the individual "leaves full-time schooling and participates in the labor force for a period of more than sixteen months without returning to full-time education during that time".[6] The choice depends on the goals and purposes of the analyst.

In this study, we are concerned with the determinants of successful transition from full-time student status to full-time labor participation. As such, we need to allow for the intermittent nature of the schooling decision by many youth by defining entry point into the labor force as the first job after the youth has last left full-time school. The data used covers six years of school and nonschool status, 1966-1971. Youth not in full-time school during at least one year in this period, as well as youth only in full-time school the entire time, were excluded. As indicated, for youth who entered and left school repeatedly, the last school exit was selected. In the empirical section, emphasis is placed on examining responses at time t, the last year in school, with responses obtained at t+1, the next year.[7]

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[3] P. M. Blau and O. D. Duncan, The American Occupational Structure (New York: Wiley, 1967); O. D. Duncan, D. L. Featherman, and B. Duncan, Socioeconomic Background and Achievement (New York: Seminar Press, 1972); Ornstein, *op. cit*.

[4] Duncan, et al., *op. cit*.

[5] A. Kohen and P. Andrisani, Career Thresholds: A Longitudinal Study of the Educational and Labor Market Experience of Male Youth, vol. 4 (Columbus, Ohio: The Ohio State University, Center for Human Resource Research, 1973).

[6] Ornstein, *op. cit*.

## Model Specification

The transition from school to work is considered in this paper as an extension of human capital theory. Usually, in such theoretical models, the individual chooses how far to continue in school, so as to maximize the discounted value of future earnings.[8] In the empirical versions of such models, individual earnings, Y, are considered a function of optimal schooling years, S, and individual "ability", A, where school years are a function of ability and market interest rates, r. With measures of A and r, such as test scores, family socio-economic status, family income and so forth, the model is recursive and exactly identified.[9]

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[7] For some youth, a one year school exit, even the last such exit, may not capture the job held after last exit, if school re-entry occurs. Within the existing data, one could require two successive years of nonschool status, but such a procedure restricts the available sample.

[8] Gary Becker, Human Capital and Personal Distribution of Income, 1967 Woytinsky Lecture (Ann Arbor, Michigan: University of Michigan, Institute of Public Administration, 1967).

[9] Sherwin Rosen, "Human Capital: A Survey of Empirical Research" in R. Ehrenberg, ed., Research in Labor Economics, vol.I (Greenwich, Conn.: JAI Press, 1977), pp.3-39.

$$S = G\,[r,A] \qquad (1)$$

$$Y = F\,[S,A] \qquad (2)$$

This is the basic model used in the present analysis. However, certain modifications are necessary since this does not allow for sufficient flexibility in an individual's combination of school/work alternatives. First, the above model implies that the youth undergoes a period of full-time schooling followed by a lifetime of full-time earnings. For many youth, such as those who face imperfect capital markets, or those who are married and maintain households away from their parents, e.g., such as a research assistant, full-time schooling is continued while holding a job. In addition, because we have selected the transition definition of the first job after last leaving school, it is quite possible that persons in school will also be active in the labor market. In contrast, the theoretical model does not allow for later wage impacts of jobs held while in school. Granted, the training impact on wages of informal on-the-job-training has been established by Mincer,[10] Becker,[11] and others; but there is a problem with Mincer's measure of potential work experience as [Age - Education - 6] in that it assumes no job was held while in school. For example, consider a twenty-year old youth with twelve years of education. If the youth did not hold a job while in school, potential experience is two years, while it is three years if a job was held during the last year in school. Because we are interested in measuring successive years in school and out of school, the problem may be minimal. Yet if schooling and informal training are substitutes in terms of their effect on future earnings, we overestimate the returns to schooling by excluding the general training content of jobs held while in school.[12]

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[10] Jacob Mincer, "On-the-Job Training: Costs, Returns, and Some Implications," Journal of Political Economy, supplement 5, part 21, vol. 70 (October 1962).

______________, "The Distribution of Labor Incomes: A Survey with Special Reference to the Human Capital Approach," Journal of Economic Literature, vol. 8, 1970.

______________, Schooling, Experience and Earnings (New York: Columbia University Press, 1974).

[11] Gary Becker, "Investment in Human Capital: A Theoretical Analysis," Journal of Political Economy, October 1962.

Ideally, one would like to include an indicator of each year in which schooling and work were combined. As an approximation, we consider the work status in the last year of schooling and distinguish full-time, part-time (less than thirty hours worked per week), unemployed, and not in the labor force. Thus, equations (1) and (2) have been rewritten as

$$S = G\,[r, A] \tag{3}$$

$$Y_{t+1} = F\,[S, A, LFS_t] \tag{4}$$

where $LFS_t$ is individual labor force status at time t when in school.

A second modification to the human capital model considers youth job search behavior. Following Stigler's seminal theoretical article on search theory,[13] several theoretical efforts have been made to examine differential wage levels for homogeneous labor at a given place and time in terms of the differential length of effort by individuals in searching for a job.[14] Search costs, time preferences in consumption, wealth, and propensity for leisure have also been empirically related to youth job search behavior.[15] According to the theory, the basic dilemma facing the youth is which of many possible offers to accept. If a low offer is accepted, search length is reduced, but subsequent earnings are also reduced. Alternatively, too high a reservation wage leads to a prolonged search period and possibly higher post-unemployment earnings. If the search for labor market information is viewed as an investment, primarily in time, certain investment optimization rules can be applied, much like those applied to school leaving decisions. That is, some youth unemployment may be beneficial in that it enables the youth to secure his highest wage consistent with his search costs. Yet, some youth unemployment may not be of this type. A previous study of mine found 90% of unemployed youth accepted their first job offer.[16] To find any offer may be a main job search goal for many youth.

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[12] This statement presumes that the job held in school is primarily part-time and that job changing occurs after schooling is completed. According to Lazear, "92% of students who work change jobs upon graduation." see Edward Lazear, "Schooling as a Wage Depressant," Journal of Human Resources, vol. 12, (Spring 1977), pp. 164-76.

[13] George Stigler, "Information in the Labor Market," Journal of Political Economy, vol. 70 (October 1962).

We incorporate this second modification by including a third and intervening equation for unemployment. Schooling and work experience in school alter the unemployment behavior and all three factors affect the wage obtained after leaving school. We now rewrite the model as

$$S = g\ [r, A, X_t, \varepsilon_1] \tag{5}$$

$$UE = H\ [r, S, LFS_t, X_t, \varepsilon_2] \tag{6}$$

$$Y_{t+1} = F\ [S, LFS_t, UE, X_{t+1}, \varepsilon_3] \tag{7}$$

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[14] Steven Lippman and John J. McCall, "The Economics of Job Search: A Survey," Economic Inquiry, vol. 14, nos. 2 and 3 (June 1976 and September 1976).

[15] Stanley P. Stephenson, Jr., "The Economics of Youth Job Search Behavior," The Review of Economics and Statistics, vol. 58 (February 1976), pp. 104-11.

[16] Stephenson, op. cit.

The $X_t$ and $X_{t+1}$ are vectors of relevant demographic and economic variables and the $\varepsilon_i$ are randomly distributed error terms with zero means that account for left out variables. Unemployment now alters earnings after school, yet the sign is ambiguous. If unemployment is a productive job search investment, then $\partial Y_{t+1}/\partial UE>0$; if, however, UE is above some subjectively determined critical point, such that reservation wage decay has substantially reduced the average acceptance wage, then $\partial Y_{t+1}/\partial UE<0$.

Two problems arise in estimating this model. Unemployment and reservation wage adjustments are dynamic concepts that we only approximate with annual survey data on weeks unemployed in the past year and the wage of the job held when interviewed. Many persons have no unemployment, which means a clustering of zero values and inefficient OLS estimation of equation (6) parameters. We, therefore, consider two measures of UE: (a) whether or not one becomes unemployed and (b) the duration of unemployment. A second issue is that search theory implies that $\varepsilon_2$ and $\varepsilon_3$ are jointly distributed with a negative covariance due to the hypothesized decline in reservation wages over unemployment duration. This means that not only will $\partial Y_{t+1}/\partial UE$ be biased, but every other term in equation (7) that is correlated with UE will be biased. To correct for this problem, we use not the actual UE measure, but an expected UE measure that considers separately (a) whether or not the youth becomes unemployed and (b) the duration of conditional unemployment. This procedure is derived with a twin linear probability model.[17]

## DATA

The study is based on the National Longitudinal Survey panel data of 5000 young men interviewed each year from 1966 to 1971. To be included, the youth had to complete two successive interviews in which he was a full-time student in year t and not enrolled in year t+1. In case of multiple periods in and out of school, the last such pair of years was selected. White and black youth were studied separately. Certain key variables were used to additionally screen the sample. That is, youth not having a recorded measure of scholastic aptitude,local unemployment rate, years of school completed, or family socio-economic status were excluded. Further exclusions were for youth with less than eight years of schooling. Sample means and standard deviations of variables used in the analysis are provided in Table 1.

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[17] Arthur S. Goldberger, Econometric Theory (New York: John Willy & Sons, 1964), p. 252.

The empirical specification of the theoretical model is a recursive system of three equations which allow us to examine direct and indirect influences on postschooling wage rates. The equations are

$$\begin{aligned}\text{Schooling} = {} & a_0 + a_1 \text{ Scholastic Aptitude} + a_2 \text{ Non-South} + \\ & a_3 \text{ Time} + a_4 \text{ Family SES} + a_5 \text{ Health Limits}_t + \\ & a_6 \text{ Family Income} + a_7 \text{ Age} \end{aligned} \tag{8}$$

$$\begin{aligned}\text{Unemployment} = {} & b_0 + b_1 \text{ Schooling} + b_2 \text{ Non-South} + \\ & b_3 \text{ Time} + b_4 \text{ UE Rate} + b_5 \text{ Marital Status} + \\ & b_6 \text{ Net Family Income} + b_7 \text{ Military Exp.} + \\ & b_8 \text{ UI Payment} + b_9 \text{ LSFT}_t + b_{10} \text{ LSPT}_t + \\ & b_{11} \text{ LSUE}_t \end{aligned} \tag{9}$$

$$\begin{aligned}\text{Hourly Wage}_{t+1} = {} & c_0 + c_1 \text{ Schooling} + c_2 \text{ Scholastic} \\ & \text{Aptitude} + c_3 \text{ Non-South} + c_4 \text{ Time} + \\ & c_5 \text{ UE Rate} + c_6 \text{ Health Limits}_{t+1} + \\ & c_7 \text{Military Exp.} + c_8 \text{ LSFT}_t + c_9 \text{ LSPT}_t + \\ & c_{10} \text{ LSUE}_t + c_{11} \text{ Unemployment} \end{aligned} \tag{10}$$

where Scholastic Aptitude is a standardized mental ability measure based on pooling scores from a variety of achievement, aptitude, and intelligence tests; Non-South is a zero-one dummy variable equal to 1 if the respondent lives in a non-south census division; Time is a linear trend measure where 1967=1, 1968=2, etc.; Family SES is an index of the socio-economic level of the respondent's parental family based on (1) father's education, (2) father's occupation when respondent was fourteen, (3) mother's education, (4) education of oldest sibling, and (5) Availability

## TABLE 1

## MEANS AND STANDARD DEVIATIONS OF VARIABLES USED IN THE ANALYSIS BY RACE

| | White | | Black | |
|---|---|---|---|---|
| Variable | Mean | Standard Deviation | Mean | Standard Deviation |
| Schooling Years | 13.62 | (2.16) | 12.52 | (1.64) |
| Scholastic Aptitude | 105.34 | (13.57) | 87.39 | (14.82) |
| Non-South Resident | .73 | ( .44) | .36 | ( .48) |
| Real Family Income/1000 | 10.36 | (6.98) | 6.69 | (4.67) |
| Real Family Income Less Respondent's Income/1000 | 7.27 | (6.86) | 4.71 | (4.11) |
| If Health Limits, time t | .06 | ( .23) | .03 | ( .16) |
| If Health Limits Work, time t+1 | .09 | ( .28) | .05 | ( .21) |
| Family Socioeconomic Status | 109.56 | (18.18) | 93.11 | (18.35) |
| If Not Married, time t+1 | .79 | ( .40) | .90 | ( .29) |
| Age When Last in School | 19.51 | (3.00) | 18.24 | (2.38) |
| Real Wage Rate per Hour | 2.13 | (1.56) | 1.70 | (1.53) |
| If Employed Full-Time, time t | .29 | ( .45) | .20 | ( .39) |
| If Employed Part-Time, time t | .33 | ( .46) | .26 | ( .44) |
| If Unemployed, time t | .07 | ( .25) | .14 | ( .35) |
| If Ever in Military | .07 | ( .25) | .03 | ( .16) |
| Local Unemployment Rate | 4.15 | (2.44) | 4.37 | (1.90) |
| Time (1=1967; 2=1968, etc.) | 3.32 | (1.78) | 2.76 | (1.61) |
| Public Unemployment Insurance Benefits per Week Unemployed | .90 | (7.46) | .34 | (2.50) |
| Predicted Conditional Unemployment Duration of Multiplied by Predicted Unemployment Incidence | 1.37 | (1.56) | 3.14 | (3.04) |
| Predicted Unemployment Incidence | .29 | ( .45) | .40 | ( .49) |
| Other Variables of Interest | | | | |
| Real Weekly Earnings, time t+1 | 93.68 | (69.61) | 69.70 | (62.32) |
| If had Positive Earnings, time t+1 | .90 | ( .31) | .90 | ( .30) |
| Average Hrs Worked per Wk, time t+1 | 38.08 | (16.56) | 36.55 | (14.94) |
| Unemployment Duration, UE>0 | 8.73 | (9.38) | 8.83 | (9.98) |

of reading material in the home when respondent was fourteen; *Health Limits* is a zero-one dummy variable equal to 1 if the respondent indicated the existence of work-limiting health problems; *Family Income* is the parental income if the individual is single or lives with his parents and is the respondent's family income for married youth not living at home (parental income, initially given in discrete intervals, was made into a continuous variable by first selecting mid-ranges, then converting the appropriate measure into constant 1967 dollars via a CPI price deflator); *Age* is the respondent's age when last in school; *UE Rate* is the local unemployment rate in the local labor market in which the respondent lived at t+1; *Marital Status* is a zero-one dummy variable equal to 1 if the respondent was not married; *Net Family Income* is family income less the respondent's annual wage or salary; *Military Exp.* is a zero-one dummy variable equal to one if the respondent ever served in the military as of time t; *UI Payment* is the annual income from public unemployment insurance divided by the number of weeks unemployed, or zero for persons never unemployed; $\text{LSFT}_t$, $\text{LSPT}_t$, and $\text{LSUE}_t$ are zero-one dummy variables which refer to the labor force status of the full-time student and indicate full-time employment, part-time employment (less than thirty work hours per week), and unemployment, respectively. The dependent variables are *Schooling*, which is measured as the highest year of school completed at t+1; *Unemployment*, which is a zero-one dummy variable equal to one if the respondent was ever unemployed in the year preceding the interview at t+1, or a continuous variable made up of the number of weeks unemployed in the past year; *Hourly Wage* at t+1, which is based on the processed response to "How much do you usually earn on this job before deductions?" and is converted into 1967 values.

Special attention in this study is placed on schooling, unemployment, and previous labor status effects on W, hourly wage. We hypothesize that schooling and previous work experience will raise W. As mentioned earlier, the unemployment variable effect on W is less clear.

Some attention is also given to the individual equations. In the schooling equation, we expect $a_1$, $a_4$, and $a_7$ to be positive as ability, taste for school, wealth, and age enter schooling decisions. The other variables control for time and place differences. In the unemployment equation, we expect $b_4$, $b_8$, and $b_{11}$ to be positive. The method of computing UI Payment, however, may add some spurious correlation to the $b_8$ estimate and, hence, $b_8$ should be considered more of an "UI eligibility" effect rather than an exact measure of UI impacts on unemployment incidence or duration. Schooling influences on unemployment are unclear. Yet, previous work experience effects, $b_9$ and $b_{10}$, are hypothesized to reduce both unemployment measures. In the wage rate equation, the direct effects of schooling, previous work experience, and scholastic aptitude are expected to be positive. Job search impacts are less clear. In one sense, successful job search investments might suggest $c_{11} > 0$; yet, reservation wage decays might lead to $c_{11}<0$. Of course, we shall also examine the indirect effects of selected variables as well. For example, the total impact on W of being unemployed during one's last year in school is $\partial W/\partial LSUE$ which is the sum of the direct effect, $c_{10}$, and indirect effects, $c_{11} \cdot \{(\partial P(UE)/\partial LSUE) \cdot (E[UE\ Duration]/\ P(UE)=1) + (\partial UE\ Duration/\partial LSUE) \cdot E[P(UE)]\}$

## RESULTS

In Table 2 are presented the full regression results and standard errors. Consider first the schooling equation results. For white youth, as expected, scholastic aptitude, family SES, and age are statistically significant and positively related to years in school. The negative and significant effect of family income was not expected. Several possible explanations are (a) nonlinear income effects (b) measurement problems created by censoring out high income responses (c) the impacts on measured low income of married college students living apart from parents. These alternatives all require further investigation. Black youth also had more years of schooling if they had a high scholastic aptitude, family SES, and were older. Family income effects is also negative for black youth, but are not significant. Also, Non-South black youth averaged nearly .4 of a year less in school years completed than southern blacks.

## TABLE 2

## REGRESSION COEFFICIENTS AND STANDARD ERRORS (IN PARENTHESES)

| | White Youth | | | |
|---|---|---|---|---|
| Independent Variable | Schooling | If UE | Weeks UE | Wage Rate per Hour[a] |
| Family SES | .02*** | | | |
| | ( .01) | | | |
| Scholastic Aptitude | .04*** | | | .04 |
| | ( .01) | | | ( .37) |
| Census Division (1=Non-South; 0=South) | - .09 | .04 | .45 | 22.15** |
| | ( .09) | (.03) | (1.36) | (10.62) |
| Family Income/1000[a] | - .02** | | | |
| | ( .01) | | | |
| Health Limits on Work, at t or t+1 (1=Yes; 0=No) | - .14 | | | -25.65* |
| | ( .18) | | | (15.60) |
| Age When Left School in Years | .47*** | | | |
| | ( .02) | | | |
| Time in Years | - .02 | .03** | .77** | 7.23** |
| | ( .02) | (.01) | ( .35) | ( 3.25) |
| Schooling | | -.01 | .19 | 20.06*** |
| | | (.01) | ( .31) | ( 2.43) |
| Marital Status | | .15*** | 2.53 | |
| | | (.04) | (2.10) | |
| Family Income Less Respondent's Income/1000[a] | | .00 | - .03 | |
| | | (.00) | ( .08) | |
| If Employed Full-Time, time t (1=Yes; 0=No) | | -.13*** | -3.88** | 66.04*** |
| | | (.04) | (1.57) | (13.77) |
| If Employed Part-Time, time t (1=Yes; 0=No) | | -.10** | -2.91** | 20.89* |
| | | (.04) | (1.36) | (12.69) |
| If Unemployed, time t (1=Yes; 0=No) | | .10* | 4.10** | 40.64* |
| | | (.06) | (1.97) | (21.59) |
| If Ever in Military (1=Yes; 0=No) | | -.06 | 4.34 | 90.40** |
| | | (.06) | (2.88) | (18.80) |
| Local Unemployment Rate, time t+1/10 | | .01** | - .01 | - .31 |
| | | (.00) | ( .02) | ( .20) |
| Unemployment Insurance Payments Divided by all Weeks Unemployed | | .01** | .04 | |
| | | (.00) | ( .04) | |
| Expected UE Duration | | | | -15.43*** |
| | | | | ( 4.64) |
| Constant | - 1.02 | .13 | 1.99 | -103.32 |
| $R^2$ | .64 | .10 | .10 | .21 |
| Mean Dependent Variable | 13.63 | .29 | 8.72 | 213.00 |
| Sample Size | 955 | 955 | 275 | 955 |

[a] In 1967 value terms for cents per hour, e.g. the mean of 213. is $2.13 per hour.

***, **, * significant at 1%, 5%, and 10% level, respectively, with a two-tailed t-test

TABLE 2 (continued)

REGRESSION COEFFICIENTS AND STANDARD ERRORS (IN PARENTHESES)

| Independent Variable | Black Youth<br>Schooling | If UE | Weeks UE | Wage Rate per Hour$^a$ |
|---|---|---|---|---|
| Family SES | .03*** | | | |
| | ( .01) | | | |
| Scholastic Aptitude | .02** | | | - .26 |
| | ( .00) | | | ( .73) |
| Census Division (1=Non-South; 0=South) | - .39** | | | 28.55 |
| | ( .17) | | | (22.60) |
| Family Income/1000$^a$ | - .01 | | | |
| | ( .02) | | | |
| Health Limits on Work, at t or t+1 (1=Yes; 0=No) | - .30 | | | -31.62 |
| | ( .49) | | | (47.74) |
| Age When Left School in Years | .39*** | | | |
| | ( .04) | | | |
| Time in Years | - .05 | -.01 | .92 | 5.50 |
| | ( .05) | (.02) | ( .76) | ( 6.93) |
| Schooling | | -.01 | - .22 | 38.04*** |
| | | (.02) | ( .82) | ( 6.93) |
| Marital Status | | .02 | 3.04 | |
| | | (.13) | (5.00) | |
| Family Income Less Respondent's Income/1000$^a$ | | .01 | - .14 | |
| | | (.01) | ( .30) | |
| If Employed Full-Time, time t (1=Yes; 0=No) | | -.24** | -2.61 | 93.25** |
| | | (.10) | (4.14) | (33.80) |
| If Employed Part-Time, time t (1=Yes; 0=No) | | -.11 | .45 | 44.98* |
| | | (.09) | (3.20) | (26.18) |
| If Unemployed time t (1=Yes; 0=No) | | .18* | 7.84** | -10.01 |
| | | (.11) | (3.10) | (58.25) |
| If Ever in Military (1=Yes; 0=No) | | .41* | 1.11 | -164.19** |
| | | (.22) | (5.60) | (80.35) |
| Local Unemployment Rate time t+1/10 | | .01 | - .05 | 1.00 |
| | | (.01) | ( .06) | ( 5.56) |
| Unemployment Insurance Payments Divided by all Weeks Unemployed | | .02 | .17 | |
| | | (.01) | ( .31) | |
| Expected UE Duration | | | | 7.38 |
| | | | | ( 7.96) |
| Constant | 1.45 | .40 | 6.75 | -358.44 |
| $R^2$ | .59 | .12 | .15 | .23 |
| Mean Dependent Variable | 12.52 | .40 | 8.83 | 170.81 |
| Sample Size | 188 | 188 | 76 | 188 |

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[a] In 1967 value terms for cents per hour.

***, **, * significant at 1%, 5%, and 10% level, respectively, with a two-tailed t-test

The second equation examines factors affecting youth unemployment incidence and conditional unemployment duration. Estimating the unemployment equations by OLS is clearly not as appropriate as some other estimation techniques, such as Tobit or logit, which accounts for the highly skewed unemployment duration distribution. That is, because 60% of the black youth and 70% of the white youth had no unemployment at all, the response surface is nonlinear, which means that the marginal impact of any variable is dependent on where on the distribution of unemployment it is evaluated. Cost constraints limited the use of the nonlinear estimation techniques. As an alternative, we use the same specification to examine separately the incidence and duration of unemployment. This procedure permits some discrimination between the marginal impacts of exogenous terms at different points on the distribution of unemployment duration.

Youth unemployment changes are probably due, in part, to aggregate demand swings, population changes, and other supply related changes, and changes in a third set of factors which are unique to youth and the labor markets they face. For white youth, we find the incidence and duration of unemployment are positively related to local unemployment rates after controlling for family income and time trend effects. The size of the effect is small, yet the positive sign suggests, perhaps, an "additional" worker effect that is puzzling.

For both race groups, unemployment insurance is seen to increase unemployment duration and incidence. But only the latter effect is significant.[18] Furthermore, since weeks unemployed entered the calculation of this variable, the estimate is negatively biased. Being interested in the transition from school to work of all young men, we cannot restrict the sample to persons holding a job prior to unemployment and calculate the ratio of UI benefits to net after tax employment earnings. We can, however consider the cost of a week of unemployment as potential earnings less weekly unemployment insurance benefits. The latter reduces the cost of unemployment and, thus, is expected to increase unemployment. Potential earning effects are approximated by schooling, census division, and previous experience gained in job holding and military experience. Of these variables, previous job holding stands out in magnitude and statistical significance, especially for white youth. To have held a job while in school raises the individual's potential earnings, which increases search costs, and, thus, reduces both the incidence and duration of unemployment. Furthermore, if full-time jobs pay more than part-time jobs, then we would expect that unemployment reductions would be greater for the former group, which is exactly what is found for whites and blacks alike. For example, for white youth who held a full-time job their last year in school, there is a 13% reduction in the probability of being unemployed relative to students who neither worked nor looked for work. The corresponding figure is 10% for holders of part-time jobs.

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[18] Ehrenberg and Oaxaca find that a UI benefit/earnings replacement ratio increase of .4 to .5 increases male youth unemployment duration by .2 weeks: see Ronald Ehrenberg and Ronald Oaxaca, "Unemployment Insurance, Duration of Unemployment and Subsequent Wage Gain," American Economic Review, vol. 66 (December 1976).

Finally, the persistence of unemployment is shown in the result that students who were unemployed when interviewed at time t experienced sharp increases in the duration and incidence of unemployment in the next twelve months.[19] The main point is that if youth unemployment reduction is a policy goal, then job holding while in school may reduce the unemployment immediately after school.

Some analysts may point out that youth unemployment in itself may or may not be wasteful. For example, if the youth via investing in unemployed job search raises his subsequent net earnings, then such youth unemployment may be privately, and even socially beneficial. We examine this possibility in the wage equation.

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[19] We checked on the possibility that some persons simply stayed unemployed for the entire year and, thus, account for the positive result. Yet only 1% to 2% of each race group had total annual UE durations of forty weeks, the maximum length.

Full regression model results by race for the hourly wage equation are presented in Table 2. Results are in terms of cents. For example, for black youth, an added year of schooling makes a direct positive marginal contribution of thirty-eight cents to the average wage per hour. Our main interest is with unemployment, schooling, and school labor market activity effects, and each is discussed in turn.

Sharp racial differences are found in unemployment impacts on the post-school hourly wage. For white youth expected unemployment duration is associated with significant and regative impact on W, whereas for black youth, a positive but nonsignificant impact was found. An added week of unemployment duration reduces the average white wage by 16 cents per added week of unemployment. Why the racial difference? Can we say unemployed search is productive for black youth? We do not know very much from the NLS data about the details of search behavior of the youth during unemployment. For example, direct marginal search cost differentials by race could account for part of the observed difference. A previous study of mine estimated weekly search costs for October 1971 of white youth at \$8.66 vs. \$3.90 per week for black youth.[20] If similar direct cost disparities are true for the NLS sample used in the present study and if search costs increase as time unemployed continues, then white youth wage demands may fall more sharply than

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[20] Stephenson, op. cit.

TABLE 3

MARGINAL CONTRIBUTION OF SELEC
HOURLY WAGE OF FIRST JOB AFTER L

| White | (1) Indirect Effect[a] |
|---|---|
| Schooling | $(-.16)\cdot[(-.01)\cdot(8.73)+(.19)\cdot(.29)]=$ |
| $LSFT_t$ | $(-.16)\cdot[(-.13)\cdot(8.73)+(-3.88)\cdot(.29)$ |
| $LSPT_t$ | $(-.16)\cdot[(-.10)\cdot(8.73)+(-2.19)\cdot(.29)$ |
| $LSUE_t$ | $(-.16)\cdot[(.10)\cdot(8.73)+(4.10)\cdot(.29)]=$ |
| **Black** | |
| Schooling | $(.07)\cdot[(-.01)\cdot(8.83)+(-.22)\cdot(.40)]=$ |
| $LSFT_t$ | $(.07)\cdot[(-.24)\cdot(8.83)+(2.61)\cdot(.40)]=$ |
| $LSPT_t$ | $(.07)\cdot[(-.11)\cdot(8.83)+(.45)\cdot(.40)]=$ |
| $LSUE_t$ | $(.07)\cdot[(.18)\cdot(8.83)+(7.84)\cdot(.40)]=.$ |

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[a] Indirect Effects of $X_i$ are computed as
$\partial W/\partial E(UE)\cdot[(\partial P[UE]/\partial X_i)\cdot(E[UE\ Duration]/P[UE]-1)+(\partial UE\ Dur$

CTED VARIABLES ON
LAST LEAVING SCHOOL

|  | (2) Direct Effect | (3)=(2)+(1) Total Effect |
|---|---|---|
| = -.01 | .20 | .19 |
| )]=.36 | .66 | 1.02 |
| )]=.27 | .21 | .48 |
| = -.32 | .41 | .08 |
|  |  |  |
| = -.01 | .38 | .37 |
| = -.22 | .93 | .71 |
| -.06 | .45 | .39 |
| .33 | -.10 | .23 |

ration/$\partial X_i$)·E(P[UE])]

black youth wage demands and the racial difference in unemployment effects on W will be found. A similar result could be found if whites continually readjust downward their wage demands as new information is obtained regarding the nature of the wage offer distribution they face; yet, there is no convincing reason why white youth should be expected to be initially further malaligned with the reality of the market than black youth. Clearly, further analysis of this result is needed.

The impact on the postschool wage of years of schooling and in-school labor market activity involve direct and indirect estimates. In Table 3 are shown such marginal effects by race. For black youth, as mentioned, an added year of schooling adds thirty-eight cents to the postschool hourly wage; however, the indirect effect is such that the total marginal impact of an added year of schooling is thirty-seven cents. Racial comparisons suggest that the marginal value of an added year of schooling for white youth is about one-half that for blacks. Part of this difference is probably due to diminishing marginal returns to continued educational investments and the fact that the years of schooling in the sample was 13.5 for whites vs. 12.5 for blacks.

Having held a job during one's last year in school both reduces unemployment and raises the postschool wage level. These effects are relatively large and statistically significant. For white youth, since unemployment is wage depressing, the direct positive wage impact of having held a full-time or part-time job is increased by the indirect effect of previous job holding on expected unemployment. For example, $\partial W/\partial LSFT_t = .66$ from Table 2 in the UE duration equation if we express the result in dollars and cents. In addition, part of the $LSFT_t$ effect on W operates through the reduction in expected unemployment duration. The total effect on W is thus (.66 +.36) or one dollar and two cents. White young men who held a full-time job during their last year in school make more than one dollar an hour more than white young men who previously neither looked for work nor held a part-time job. Further such derivations are presented in Table 3.

Part-time job holding has about one-half the full-time job impact on W and merely having looked for work during one's last year in school may have some payoff relative to NLF students. Racial patterns are similar in terms of the relative marginal influence of previous full-time and part-time job holding. However, in school job holding increases the wage of white youth more than for black youth. Another racial difference arises because black unemployment is wage-augmenting, whereas, for white youth unemployment is wage depressing. The net wage gain to white youth who were unemployed in school is eight cents per hour over NLF youth versus the twenty-three cent differential for black youth.

SUMMARY

We began the analysis by considering the issue of how to define the transition from school to work. In a range of definitions we chose the first job after last leaving school. By selecting this point, we increased the average observed age and also increased the possibility that the line between school and work would become increasingly blurred. This point was selected, however, not to obfuscate the transition issue, but to choose that point that best marks a permanent leaving of full-time student status. Such a point may best serve the need to examine jobs leading to lifetime careers.

The analysis was considerably aided by the availability of the rich NLS panel data on young men. However, sample attrition, nonresponse to key questions, and the transition process selected all served to limit the final sample studied. Since the data covered the period from 1966 to 1971, military experience and time trend variables were added as control variables.

The major results of the analysis were obtained in an expanded human capital model. The expansion was (a) to include unemployment incidence or duration as an intervening equation between the standard schooling and wage rate equations and (b) to consider the impact on unemployment and wage rates of holding a job or looking for a job while in school. Job holding in school reduces later unemployment for white and black youth. Furthermore, previous job holding, especially full-time job holding while a student, sharply increases hourly wage rates on postschool jobs.

In terms of job search implications during the transition process, racial differences were found. In the sample, 40% of the black youth had some unemployment. For such youth, post-unemployment wage rates were higher than for other black youth. White youth, however, who experienced unemployment , had lower subsequent wages. While further research is needed regarding this important issue, the preliminary policy implication is that unemployment may not be either socially or privately wasteful to some black youth and that a policy of in school work experience might aid the transition process for both groups.

# THE ESTABLISHMENT OF STABLE AND SUCCESSFUL EMPLOYMENT CAREERS: THE ROLE OF WORK ATTITUDES AND LABOR MARKET KNOWLEDGE

By: Paul J. Andrisani

## ABSTRACT

This paper assesses the extent to which stable and successful employment careers are a function of the work attitudes and labor market knowledge of youths. The paper examines the work attitudes and labor market knowledge of youths, considers how they differ from their older and presumably more mature counterparts, and how they affect and are affected by successes and failures upon initial entry into the work force. The research summarized draws primarily from published works based on the National Longitudinal (Parnes) Surveys.

The data summarized clearly show the importance of positive work attitudes and adequate labor market knowledge for the establishment of stable and successful employment careers. Confidence in these findings is strengthened by the fact that the observed relationships were independent of individual differences among the youths studied in a wide range of skills, abilities, and demographic characteristics, and were supported by longitudinal data as well. The data also document persuasively the inadequacy of all types of labor market knowledge as youths cross the critical threshold from school to work--with blacks, females, and poor white youths possessing the least adequate knowledge. Yet, the data provide little empirical justification to consider youths' attitudes toward work as inadequate, immature (other than for lack of adequate labor market information), anti-work, or the cause of the unique labor market problems of youth. On the basis of the statistics for the late 1960s, youths' attitudes toward work were hardly dissimilar from those of older workers; they reflected considerable ambition in the setting of career goals, and they were shown to be influenced in an anti-work direction by unsatisfactory labor market experiences early in work careers.

## INTRODUCTION

Referring to out-of-school and unemployed teenagers, Charles Silberman once wrote:

> Many of them appear to be unemployable: they are--or seem to be--uninterested in working, unwilling or unable to adjust to the routine and discipline of a job, and generally apathetic, sullen or hostile...teenagers also lack the psychological pressures that make the great majority of adult men prefer work to idleness...holding down a job

> is not necessarily a source of status, nor is unemployment a source of shame. On the contrary, in at least some city slums, teenage society displays a certain disdain for legitimate work.[1]

Quite similarly, on the basis of The Coleman Report's finding that a youth's attitude "was more highly related to achievement than any other factor in the student's background or school,"[2] James S. Coleman was prompted to write:

> ...internal changes in the Negro, changes in his conception of himself in relation to his environment, may have more effect on Negro achievement than any other single factor. The determination to overcome relevant obstacles, and the belief that he will overcome them...may be the most critical element in achieving equality of opportunity.[3]

Still further, Edward Kalachek's analysis of the youth labor market has also noted the potential importance of certain work attitudes for success in the world of work:

> Reports of operating manpower agencies stress the importance of attitudinal factors...Clients drawn from these groups are frequently characterized as alienated, discouraged, immature, lacking self-esteem, and not conversant with accepted middle-class work values. Counseling, efforts at building self-esteem, and emotionally supportive services are cited as essential elements in improving employability.4

Another potentially important but seldom researched factor in establishing stable and successful work careers is the adequacy of

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1. C. Silberman, "What Hit The Teenagers?," Fortune, April 1965, as cited in E. Kalachek, The Youth Labor Market (Washington: National Manpower Policy Task Force Policy Papers in Human Resources and Industrial Relations No. 12, 1969), p. 74.

2. J.S. Coleman, "Equal Schools or Equal Students?" in D.M. Gordon, ed., Problems in Political Economy: An Urban Perspective (Lexington: D.C. Heath and Co., 1971), p. 193, emphasis in original.

3. Ibid.

4. Kalachek, op. cit., p. 77.

labor market knowledge available to youths.[5] Inadequate labor market knowledge is conceptually linked to work attitudes, since it implies a faulty perception of the operation of the labor market. When the parents, relatives, friends, teachers, or guidance counselors of youths--and of female and disadvantaged youths in particular--possess inadequate labor market knowledge, not only may this develop faulty perceptions among the youths, but it may shape important work attitudes in an anti-work direction as well.

It may, for example, preclude youths from adequately preparing for the careers to which they aspire. Or, as has long been the case with young women, it may cause them to aspire to and prepare for careers that they may later learn were not what they wanted or expected, or that were not readily available. In each instance, inadequate labor market knowledge may generate unrealistic career aspirations and expectations on the part of youths that are easily dashed when confronted with the harsh and oftentimes brutal realities of the world of work. The consequences for the ultimate establishment of stable and successful work careers quite obviously may be serious. In an economic system in which youths are presumably free to choose among various occupational and employment opportunities, the freedom to choose is indeed hollow in the absence of adequate labor market knowledge.

The principal purpose of this paper is to assess the extent to which stable and successful employment careers are a function of the work attitudes and labor market knowledge of youths.[6] A secondary purpose is to examine what the work attitudes of youths actually are, how they differ from those of their older and presumably more mature counterparts, and how they are affected by successes and failures upon initial entry into the work force. To a considerable degree, the research summarized in this paper draws upon published works from a unique set of longitudinal data for representative national samples of 10,000 American youths and 10,000 older workers during the second half of the 1960s--namely, the National Longitudinal Surveys.[7]

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[5] H.S. Parnes and A.I. Kohen, "Occupational Information and Labor Market Status: The Case of Young Men," Journal of Human Resources, vol. 10, no. 1 (Winter 1975), pp. 44-55. See also: G. Stigler, "Information in the Labor Market," Journal of Political Economy, vol. 70, no. 5 (Supplement, October 1962), pp. 94-105.

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[6] That is, the extent to which stable and successful employment careers are a function of work attitudes and labor market knowledge rather than of traditional human capital and demographic variables most typically considered in labor market studies. These include such variables as age, race, sex, education, general and specific training, institutional and on-the-job training, health, marital status, region of residence, type of local labor market, etc. In the human capital model of income distribution developed by Becker and Mincer, labor market success, particularly in terms of earnings, is a function of investments in an individual's productive skills and abilities, comprising one's stock of human capital. Variables listed above that do not reflect human capital or supply-side characteristics, are generally presumed to reflect demand-side or labor market effects. See: G. Becker, Human Capital (New York: Columbia University Press, 1964)and J. Mincer, "The Distribution of Labor Incomes: A Survey with Special References to the Human Capital Approach," Journal of Economic Literature, vol. 8, no. 1 (March 1970), pp. 1-26.

[7] Begun in 1966, the NLS is a ten-year longitudinal study of the work experience of four age-sex cohorts of the population, each of which consists of roughly 5,000 respondents: men forty-five to fifty-nine, women thirty to forty-four, and young men and women fourteen to twenty-four years of age at the date of the initial interviews. The samples in each case constitute a representative national probability sample of the noninstitutionalized civilian population of the particular cohort as of the first survey date. The samples were drawn and personal interviews conducted by the U.S. Bureau of the Census for The Ohio State University Center for Human Resource Research under separate contracts with the U.S. Department of Labor. For an overview of the entire NLS data base, including a complete description of the sampling design, interviewing procedures, and the host of included variables, see: The National Longitudinal Surveys Handbook (Columbus: The Ohio State University Center for Human Resource Research, 1975).

In the section to follow, the adequacy of labor market knowledge available to youths and the consequences of inadequate knowledge will be explored. Then in the third section, the paper will explore what the work attitudes of youths actually are, and how they differ from those of older workers. In the fourth section, the empirical evidence relating to whether the work attitudes of youths have any bearing on their early labor market experience will be examined. The fifth section then examines the empirical evidence relating to the effects of early labor market experience on work attitudes, and the final section concludes with a brief discussion of the implications of the research reported herein.

## THE ADEQUACY AND IMPORTANCE OF LABOR MARKET KNOWLEDGE

According to Parnes, there are four types of labor market knowledge that should be considered in discussions of this sort.[8] The first and second are essentially the distinction between "general" and "specific" human capital investments made by Becker.[9] Characteristically, general labor market information is a highly substitutable good with a market of substantial breadth. It comprises, for example, short run as well as long run knowledge of alternative occupations, industries, geographic locations, relative income and job security prospects, avenues of preparation for various jobs, and a host of other characteristics of the alternative opportunities available in the labor market. In contrast, specific labor market information in its most limited form is the opposite side of the coin--i.e., at the limit it is confined to a single market and to even a single firm.

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[8] H.S. Parnes, "Improved Job Information: Its Impact on Long-Run Labor Market Experience," in Seymour L. Wolfbein, ed., Labor Market Information for Youths (Philadelphia: Temple University School of Business Administration, 1975), pp. 163-83. Much of the material in this section of the paper summarizes a recent conference on Improving Labor Market Information for Youth held at Temple University on October 21-22, 1974. The proceedings were published in the volume cited here.

[9] Becker, op. cit.

Clearly the former is more important than the latter in preparing youths for the world of work. It may be the latter, however, which is of relatively greater importance to youths after adequately preparing for work--i.e., during the critical period of settling upon a specific firm.[10]

The third type of labor market information includes knowledge of the relative advantages and disadvantages of various job search strategies and institutions--e.g., public and private employment services, trade unions, and various public and private social welfare agencies. Finally, the fourth type of labor market information of relevance to workers, particularly youths, is what Parnes calls "an understanding of the regimentation that is inherent in greater or lesser degree in most work situations."[11] This understanding includes knowledge of the "importance to success of regular attendance, punctuality, good work habits, concern for the objectives of the employing establishment, and conformity to accepted standards of dress and behavior."[12] Irrespective of whether one endorses all of these or whether all are essential to satisfactory performance on all jobs, it is Parnes' view that failure to alert youths to such facts of industrial life would indeed be a disservice to them.

Whichever type of labor market knowledge is considered, there is persuasive evidence that many youths take their first jobs in a very haphazard manner, and that initial decisions often have a lasting impact.[13] Mangum estimates that more than one million youths initially entering the labor force each year encounter difficulty assimilating into the world of work, with blacks considerably overrepresented within this group.[14] In the very tight labor market of 1967, for instance, the average rate of unemployment among nonwhite teenagers was 26.5%, a level seven times the national average of 3.8%.[15] Furthermore, there is little doubt of the inadequacy of all types of labor market knowledge among youths--particularly blacks, young women, poor whites, and those in rural labor markets.[16] Singell's follow-up of high school graduates in Detroit a year after initial entry into the labor market is typical in its observation that:

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[10] There is considerable evidence, for example, that the differential in unemployment rates between adults and youths results mainly from greater turnover, and thus a higher incidence of unemployment. See M.S. Feldstein, "Lowering the Permanent Rate of Unemployment," a study prepared for the use of the Joint Economic Committee, Congress of the United States, 93rd Congress, 1st Session, September 1973 (Washington: U.S. Government Printing Office, 1973), pp. 17-18.

[11] Parnes, op. cit., p. 167.

[12] Ibid.

> Most youths had not "chosen" a job in any real sense, but had either drifted into one or had taken it because they could find no other...Furthermore, the youths exhibited extremely vague knowledge about wages,

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[13] Ibid., p. 169. Also see, for example: P.J. Andrisani, An Empirical Analysis of the Dual Labor Market Theory (Columbus: The Ohio State University Center for Human Resource Research, 1973); P.E. Davidson and H. Anderson, Occupational Mobility in an American Community (Stanford: Stanford University Press, 1937); M.D. Ornstein, Entry into the American Labor Force (New York: Academic Press, 1975); J. Piker, Entry into the Labor Force: A Survey of Literature on the Experiences of Negro and White Youths (Ann Arbor: Institute of Labor and Industrial Relations, University of Michigan--Wayne State University, 1969); Kalachek, op.cit.; and L. Reynolds, The Structure of Labor Markets (New York: Harper and Brothers, 1951).

[14] G.L. Mangum, "Second Chance in the Transition from School to Work", in P. Arnow et al., eds., The Transition from School to Work, (Princeton: The Industrial Relations Section, Princeton University, 1968), pp. 231-69.

[15] P. Arnow et al., "The Transition from School to Work", in The Transition from School to Work, op.cit., p.3.

[16] Parnes and Kohen, op. cit.; P. Brito and C. Jusenius, "Occupational Expectations for Age 35", in F.L. Mott et al., eds., Years for Decision, IV, (Columbus: The Ohio State University Center for Human Resource Research, 1977), pp. 113-139.

> working conditions, steadiness of employment, and chances of advancement when they accepted their first job.[17]

Years earlier, Reynolds' classic study of the New Haven labor market produced results that were essentially the same: "Most youngsters (and their parents) approached the choice of a first job with no clear conception of where they were going..."[18] Other studies from Davidson and Anderson in 1937 to a very recent and comprehensive one by Michael Ornstein are equally consistent on the point.[19] When a sample of sixty-nine vocational educators in twenty-two urban areas were asked to discuss the labor market problems of high school graduates, for instance, more than half reported that youths have unrealistic aspirations and expectations concerning work, and more than 40% felt that poor attitudes and lack of responsibility, maturity, and self-disipline were part of the problem.[20]

Furthermore, there is little reason to be any more sanguine about the adequacy of labor market information available to postsecondary students. Perhaps the only study to be impressed by the adequacy of labor market knowldege of any type is that of Richard Freeman.[21] As Parnes has noted, however:

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[17] L.D. Singell, Some Private and Social Aspects of the Labor Mobility of Young Workers", The Quarterly Review of Economics and Business, vol. 6, no. 1 (Spring 1966), p. 23.

[18] Reynolds, op. cit.,pp. 213-14.

[19] Davidson and Anderson, op. cit.; and Ornstein, op. cit.

[20] A.P.Garbin, R. Campbell, D.P. Jackson, and R. Feldman, Problems in the Transition from High School to Work as Perceived by Vocational Educators (Columbus: The Ohio State University Center for Vocational and Technical Education Research Series No. 20, 1967), pp. 49-50.

[21] R. Freeman, The Market for College-Trained Manpower: A Study in the Economics of Career Choice (Cambridge: Harvard University Press, 1971).

> ...even Freeman's data show that a sixth of the undergraduates and an eighth of the graduate students believed that the information available to them at the time they made their career choices was inadequate, while another fourth regarded it as "barely adequate". Moreover, other investigators of the process of career choices among college students have drawn generalizations different from those of Freeman. A study of almost two thousand male and female members of the 1972 graduating classes of five Pennsylvania colleges and universities reported that...two-fifths of the students reported that they were "not too aware" and one-fifth that they were "not at all aware" of the job market in the field of their major at the time they selected it.[22]

Of what empirical consequence are such differences in labor market knowledge among youths? One of the very few studies to examine the issue is that of Parnes and Kohen.[23] Using longitudinal data on male youths from the National Longitudinal Surveys, a multivariate statistical model, and a multi-item measure of occupational information, Parnes and Kohen found that scores in 1966 on the NLS measure of labor market knowledge were significantly correlated with the hourly wage rates and occupational attainment of the youths two years later. Moreover, the results were obtained for young black men and their young white counterparts, with statistical controls for such human capital and demographic variables as years of schooling, IQ, socio-economic status, quality of high school attended, years of work experience, health status, region of residence, and rural versus urban residence. As these researchers correctly observed:

> Indeed, there is reason to believe that these findings understate the total contribution of the kinds of labor market information measured by the test, since they ignore the indirect effects such knowledge may have on occupational status and wages via its effect on educational attainment.[24]

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[22] Parnes, op. cit., p. 171.

[23] Parnes and Kohen, op. cit.

[24] Parnes, op. cit., pp. 175-76.

Given the empirical results, what then would be the social consequences of improved labor market knowledge? Would it simply mean a reshuffling of youths among jobs as the gains of those with superior labor market knowledge are transferred to those youths with inadequate knowledge? Theoretically, the answer to the second question is no, since improved labor market knowledge implies movement of the labor market from disequilibrium to equilibrium--with the resultant reallocation of labor being more nearly optimal as youths and older workers become better matched with the jobs for which they are best suited. In theory, an optimal allocation of human resources means that no one could improve his or her utility or contribution to the total social product by making any job change. Thus, the reallocation should lead to increased productivity and individual welfare. Second, a more equitable distribution of labor market knowledge among youths should reduce race, sex, and class differences in unemployment and income, since greater labor market knowledge has been shown in the NLS data both to be disproportionately possessed by white, male and higher SES youths and to be significantly linked with a successful transition from school to work.

Third, improved labor market knowledge would enhance the operation of competitive forces in the labor market. Thus, for example, some employers would feel a stronger need to improve the quality of work to keep employees, and the labor market would be better able to reallocate labor more efficiently in response to technological shifts and to shifts in consumer preferences and tastes. Increased competition should act to reduce levels of unemployment and to shift the Phillips curve down to the left. The latter result might be particularly effective for reducing youth unemployment, since it would reduce youth turnover and frictional unemployment which play so large a role in the differential between youth and adult unemployment rates.[25]

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[25] Feldstein, op.cit., pp. 17-18.

## WHAT ARE THE ATTITUDES OF YOUTHS TOWARD WORK AND ARE THEY DIFFERENT FROM THOSE OF THEIR ELDERS?

By way of the data for the four NLS cohorts covering the 1966-1972 period, it is possible to examine a number of the work attitudes of male and female youths and to compare them with those of their older and presumably more mature counterparts. When respondents who were in the labor force at the initial surveys were asked whether they would continue to work if by some chance they were to get enough money to live comfortably without working, 73-78% of the older men, 78-82% of the younger men, 58-68% of the older women, and 59-70% of the younger women replied that they would continue to work.[26] The high degree of reported commitment to work and the absence of age differences in responses to this question are entirely consistent with responses to eleven other attitudinal items more directly probing commitment to the Protestant work ethic. For each of the eleven items there tended to be virtually no age differences in the tendency to ascribe credence to the Protestant work ethic belief that hard work leads to success.[27]

With respect to their occupational goals, young men and women in 1966 aspired to considerably greater occupational heights than either they or their older counterparts had attained.[28] Two to three years later, as the youths aged and gained more work experience, occupational aspirations in the aggregate changed little for white or black, male or female youths. Coded in terms of the Duncan index of occupational status, white young men in 1966 aspired to occupations averaging 55 points (on a scale from 0 to 100), while black young men aspired to occupations averaging only 46 points. Among the young women, whites in 1968 aspired to jobs averaging 53 points, while blacks aspired to jobs averaging 48 points. Of the youths who worked at each survey date, and who were not enrolled in school, the actual occupational attainments of the white and black young men were 32 and 19 points respectively, in 1966, and 44 and 30 points respectively for white and black young women. Of those employed in the two older NLS cohorts at each survey date, the actual occupational attainments of the white and black men were 40 and 18 points respectively, and 43 and 29 points respectively for white and black women.

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[26] This portion of the paper draws heavily from Chapters 3,4, and 5 of a research volume on the relationship between work attitudes and labor market experience: P.J. Andrisani, with the assistance of E. Applebaum, R. Koppel, and R.C. Miljus, Work Attitudes and Labor Market Experience: Evidence from the National Longitudinal Surveys (New York: Praeger Publishers, 1978).

[27] P.J. Andrisani, "Internal-External Attitudes, Personal Initiative, and Labor Market Experience," in Andrisani et al., op. cit., chapter 4.

[28] P.J. Andrisani, "Work Attitudes and Labor Market Experience: Other Findings," in Andrisani et al., op. cit., chapter 5.

Yet while aspirations were high, 29% of the young white men and 37% of the young black men perceived their chances of attaining their goals as only "fair" at best.[29] Furthermore, the gap between aspirations and actual occupational attainment was much greater for black youths than white youths. Relative to their actual occupational attainment, or relative to the occupational attainment of their counterparts in the older cohorts, black youths aspired to greater heights than white youths. Within race groups, however, young men and women differed little on the average in their occupational goals. Thus, while blacks and female youths may be unrealistic in their occupational goals, they can hardly be accused of lacking ambition in setting their goals.

When asked whether "wages" or "liking the work" was the more important aspect of a good job, better than half within nearly every NLS age-sex-race group responded "liking the work."[30] Only among black men were preference differences between the younger and older NLS cohorts more than four percentage points, and even in the one instance the age differences are hardly suggestive of substantially different work attitudes between youths and their elders. When asked to report the aspects of their jobs that they liked best, white and black, male and female youths were most inclined to mention "the work itself."[31] Other frequently mentioned factors were economic aspects of work and interpersonal relationships. On balance, their responses were not dissimilar to those of older workers. Much the same, when asked to cite the specific aspects of work disliked most, youths reported a wide range of factors, ultimately constituting patterns quite similar to those reported by their elders.

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[29] Ibid.

[30] Ibid.

[31] Ibid.

How satisfied were youths with their jobs, particularly in relation to the reported job satisfaction of their older counterparts? Remarkably few youths expressed a disliking for their jobs at any survey date between 1966 and 1972.[32] Fewer than 15% of the employed respondents within any of the NLS age-sex-race groups reported that they disliked their job somewhat or disliked it very much. At some points during the period, however, age differences in degree of job satisfaction did tend to be substantial, particularly between young and middle-aged black men. For example, in 1966 only 34% of young black males who were out of school and working reported themselves to be highly satisfied with their jobs, in contrast to 51% among their older counterparts. For the most part, age differentials in levels of job satisfaction narrowed during the 1966-1972 period.

In sum, with the possible exceptions of their occupational aspirations and degree of job satisfaction, the NLS data provide little evidence that the work attitudes of youths are essentially different from those of their older counterparts. It also is at odds with the data to suggest that youths, particularly young blacks and young women, lack ambition in setting their employment goals. That their goals tend to be so high may be a cause for concern, especially for the adequacy of their labor market knowledge. It is quite possible that at least a part of the job dissatisfaction, high turnover, and high unemployment rates of youths reflects their high--and perhaps unrealistic--occupational aspirations, which in turn may reflect the inadequacy of their labor market information.

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32. P.J. Andrisani, "Levels and Trends in Job Satisfaction, 1966-1972," in Andrisani et al. _op. cit._, Chapter 3.

## THE EFFECTS OF WORK ATTITUDES

The NLS data provide clear evidence of the importance of work attitudes in conditioning subsequent labor market success among youths.[33] For example, the relationship between job dissatisfaction and turnover is unmistakable, suggesting that highly dissatisfied youths were from eighteen to forty-two percentage points more likely than comparable youths who were highly satisfied with their jobs to change employers subsequently.[34] Moreover, since youths stand less to lose, the data not suprisingly show a stronger relationship between job dissatisfaction and turnover among youths than older workers. The evidence also suggests that job dissatisfaction imposes considerable costs on youths in terms of increased unemployment, decreased labor force participation, and below-average growth in annual earnings. Furthermore, the data show that the costs of job dissatisfaction reflect more than the costs of turnover which were borne disproportionately by dissatisfied youths. Among comparable white youths who did not change employers, there is considerable evidence that those dissatisfied with their jobs were below-average in occupational and earnings advancement. Among those who changed employers, the dissatisfied were generally above-average in weeks of unemployment as well. The evidence thus suggests that job dissatisfaction often leads to reduced productivity and to job changing that has been less carefully planned than that which takes place among comparable youths more highly satisfied with their jobs.

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33. P.J. Andrisani, "Introduction and Overview," in Andrisani et al., op. cit., Chapter 1. To assure that workers were comparable regressions were performed separately for each of the eight NLS age-sex-race groups with control variables for such human capital and demographic variables as education, formal training, years of work experience, seniority with employers, health, marital status, region of residence, and type of local labor market.

34. P.J. Andrisani, "Differences Between Satisfied and Dissatisfied Workers in Subsequent Labor Market Experience," in Andrisani et al., op. cit., Chapter 2.

The NLS data also provide strong and consistent evidence that male youths with an "internal attitude"--i.e., who perceive payoffs to their initiative--*subsequently* experience greater labor market success than their contemporaries who perceive less payoffs to their efforts.[35] *The Coleman Report* found that differences in these attitudes were a more important factor in explaining achievement among black youths than all of the differences in school quality and family background combined. Internal-external attitudes are also of interest because of a similarity to the concept of alienation, and because some have maintained that racial differences in labor market experience stem mainly from racial differences in work ethic attitudes closely resembling the internal-external distinction.

While the NLS data suggest that attitudinal change among youths who perceive little payoff to their personal initiative would result in greater initiative and more successful labor market experience, there is little reason to suspect that the elimination of age differences in these attitudes would have an appreciable impact upon age differences in work experience. In the main, youths differ only minimally from older workers in these attitudes.

More specifically, the NLS data show that white and black young men with an internal outlook in 1968 were in the better occupations and had higher hourly earnings two years later than comparable youths with an external attitude. In the regressions which come closest to identifying a true causal relationship, those examining subsequent growth in earnings and occupational advancement, internals of both race groups were also more likely to outdistance comparable externals both in terms of growth in hourly earnings and occupational advancement.

As for the magnitude of the relationships, those male youths who were "slightly internal" in 1968, a score of eight on the attitudinal measure, were estimated to have enjoyed a 12% differential in hourly earnings two years later over comparable youths who were "slightly external" in outlook, a score of twelve. Also, their average hourly earnings were estimated to have advanced by $.20 per hour more between 1968 and 1970 than the wage rates of comparable youths who were "slightly external."

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[35] Andrisani, "Internal-External Attitudes, Personal Initiative, and Labor Market Experience", *op. cit.*

With respect to racial differences, there were only two cases where the regression coefficients differed between white and black young men. In the first, it appears that these attitudes are more strongly related to annual earnings two years later for whites than blacks. In the second, it appears that the attitudes are more strongly related to occupational advancement for blacks than whites.[36] Moreover, the elimination of racial differences in internal-external attitudes among youths apparently would not appreciably reduce racial differences in their labor market experience, since these are only slight differences in these attitudes between white and black youths of either sex.

The only aspect of labor market experience examined with the NLS data that was not significantly related to internal-external attitudes for either white or black young men is growth in annual earnings. Although the data do not address the issue directly, this may reflect greater investments (foregone earnings) that youths with an internal attitude have made in order to realize their advantage two years later in hourly earnings and occupational attainment. Greater investments in job search and mobility, in particular, may explain why youths with an internal attitude outdistanced comparable youths with external outlooks in growth in hourly earnings and in occupational advancement without exceeding their growth in annual earnings.

Differences among otherwise comparable workers in other work-related attitudes--e.g., career goals, expectations of achieving goals, commitment to work, attitudes toward one's work role, attitudes toward propriety of working mothers, and spouse's attitude toward one's work role--have also been shown with the NLS data to bear a relationship to subsequent labor market experience.[37] In particular, the career goals of male youth, both black and white, and a belief that one's goals are attainable, are linked to a successful transition from school to work.[38] The NLS data show that white and black male youths with greater occupational aspirations in 1966 advanced more in annual earnings over the next three years than comparable youths who were less ambitious. Among the white youths, occupational aspirations were also related to occupational advancement, to advancement in hourly rates of pay, to the reception of formal occupational training, and to fewer weeks of unemployment. Among the black youths, the more ambitious were also more inclined to receive formal occupational training, but there is no evidence that they were more likely to advance occupationally, or in wage rates, or that they were any less likely to encounter unemployment. One likely explanation why the aspirations of black youths are not as closely linked to subsequent labor market success as for whites is of course that employers discriminate against blacks. Additional or alternative explanations include the fact that the black youths are more deficient in labor market information.

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[36] Ibid.

Other regression results with the NLS data for male youths reinforce these findings. White and black young men with greater self-confidence had considerably greater subsequent success in the labor market during the transitional period from school to work than comparable youths with less self-confidence. Those black and white young men who perceived their chances of attaining their career goals as "excellent" in 1966, for example, advanced occupationally from seven to eight points more (on the Duncan

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[37] Andrisani, "Work Attitudes and Labor Market Experience: Other Findings," op. cit.: and E. Appelbaum and R. Koppel, "The Impact of Work Attitudes Formed Prior to Labor Market Entry on the Process of Early Labor Market Attainment," in Andrisani et al., op. cit., Chapter 6.

[38] Andrisani, Ibid.

Index) over the next three years than comparable youth who initially rated their chances as "poor". In addition, their annual earnings advancement over the next three years exceeded that of those less confident by $825 to $1150, and the more confident youths experienced three to four fewer weeks of unemployment as well. Among the white youths, the more confident ones also had considerably greater advancement in hourly rates of pay, fewer weeks out of the labor force, and were more prone to be geographically mobile.

It thus seems clear on the basis of the NLS data that the work attitudes of youths are important factors in understanding the process of establishing stable and successful employment careers. The NLS results summarized above show that the effects of the work attitudes of youths are independent of the effects of a wide range of more traditional labor market variables--e.g., human capital and demographic variables. They also show that the attitudes have effects on subsequent labor market experience, thus showing that cross-sectional relationships between work attitudes and labor market experience do not reflect exclusively the influence of the latter on the former. Indeed, there is reason to believe that these findings may understate the real effect of work attitudes on the labor market experience of youths, since it has not been possible to isolate the indirect effect of the attitudes on experience via their effect on investments in education and other forms of human capital. Nor has it been possible to estimate the extent to which the effects are understated by using such crude and simplistic, and thus inherently less reliable, measures of work attitudes as are available in the NLS.

Finally, it should also be noted that these findings are quite consistent with those of other studies of youths from more restricted samples. According to Sewell and Hauser's study of Wisconsin youths, for instance:

> . . . the inclusion of the social psychological variables has resulted in a more complete explication of the attainment process in the educational, occupational, and economic spheres . . . The expanded model we have discussed in the present chapter has illuminated the rather complex process by which the effects of socioeconomic

> background on educational, occupational, and economic attainments are mediated by various social psychological experiences. [39]

## THE EFFECTS OF LABOR MARKET EXPERIENCE ON THE WORK ATTITUDES OF YOUTHS

Recent studies with the NLS data for youths have examined the extent to which labor market experiences early in work careers affect the occupational aspirations of youths.[40] In one of them, changes in occupational aspirations among the young men between 1966 and 1969 have been regressed on their occupational status and hourly earnings between 1966 and 1969.[41] In addition, to assure that comparable young men were being considered, control variables for individual differences at the beginning of the period were also included in the regressions--e.g., for years of schooling, completion of formal occupational training, years of general on-the-job training, years of service with 1966 employer, health status, marital status, region of residence, degree of urbanization in the local labor market, and initial levels of occupational aspirations, work commitment, and preferences for noneconomic versus economic rewards.

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39. W. H. Sewell and R. M. Hauser, "Social Psychological Factors in Achievement," in W. H. Sewell and R. M. Hauser, Education, Occupation, and Earnings: Achievement in the Early Career (New York: Academic Press, Inc., 1975), pp. 111 and 90. See also: R. M. Gasson, A. O. Haller, and W. H. Sewell, Attitudes and Facilitation in the Attainment of Status (Washington: The American Sociological Association Rose Monograph Series, 1972); A. C. Kerckhoff, Ambition and Attainment: A Study of Four Samples of American Boys (Washington: The American Sociological Association Rose Monograph Series, 1974); A.O. Haller and A. Portes, "Status Attainment Process," Sociology of Education, vol. 46 (Winter 1973), pp. 51-91; and R. C. Edwards, "Individual Traits and Organizational Incentives: What Makes a 'Good' Worker?" Journal of Human Resources, vol. 11, No. 1 (Winter 1976), pp. 51-68.

40. Brito and Jusenius, op. cit.; Andrisani et al., op. cit., Chapter 5; and J. Grasso with A. I. Kohen, "The Formation and Revision of Goals by Young Men," in A. I. Kohen et al., Career Thresholds, IV, (Columbus: Ohio State University Center for Human Resource Research, 1977), pp. 15-52.

41. Andrisani et al., op. cit., Chapter 5.

Among white and black young men, as previously noted, there were virtually no changes in occupational aspirations, on the average, between 1966 and 1969, despite the fact that many youths crossed the critical threshold from school to work during the period. For young men who were employed as wage and salary workers and not enrolled in school during the 1966-1969 period, the subset of the total cohort included in these regressions, aspirations on the average changed by only two-thirds of one point among the white youths and by less than one-twentieth of one point among the black youths. Nonetheless, the evidence suggests that more favorable work experience leads to more favorable work attitudes and that unsuccessful work experience leads to discouragement and reduced aspirations. Among both race groups, for example, those in the better occupations initially, and those who advanced the most occupationally during the period, raised their occupational aspirations between 1966 and 1969, while those in the lowest status jobs initially and those who advanced the least or regressed occupationally reduced their aspirations between 1966 and 1969. Furthermore, among black youths, those in the better paying jobs initially and those whose earnings grew the most during the period became more ambitious in career objectives between 1966 and 1969, while those in the low-paying jobs initially and those whose earnings advanced the least reduced their occupational goals during the period. The influence of earnings was so strong, in fact, that those black youths who differed by $1.00 in initial hourly earnings or in growth in hourly earnings, differed in changes in their occupational aspirations by 5.5 and 3.4 points respectively on the Duncan Index.

Other variables also related to changing aspirations included years of work experience, health, and suburban residence. The longer the youths had been out of school in 1966, and hence the greater their experience in the world of work, the less likely they were to revise their aspirations upward and the more likely they were to revise them downward. Among blacks, those with a health problem in 1966, whose career goals at that point may have been most seriously in doubt, raised their aspirations by fourteen points more during the period than comparable youths with no health limitation in 1966. Among whites, those residing in suburban areas raised their aspirations by three points more than those youths residing in rural or central city locations.

Other published data from the NLS have shown that family background, ethnic origin, schooling, and the extent of a youth's labor market information have a considerable impact on occupational aspirations while youths are still in school.[42] Still further, Brito and Jusenius show that occupational goals of the NLS young women were significantly related to the young women's family backgrounds and education, as well as to their actual labor market experience.[43] Quite similarly, Grasso and Kohen report that the occupational goals of NLS male youths enrolled in high school in 1966 were significantly linked to their labor market knowledge and IQ, and--in the case of the whites only--to their family backgrounds.[44] In their own words:

> ...measures of information (e.g., amount of schooling, availability of reading material in the home) and of opportunities and constraints (e.g., encouragement by parents, school personnel and peers) also contributed to explaining variations in levels of aspiration ... While growing up in a large urban area was associated with decidedly higher educational and occupational goals, the evidence is that these goals are likely to be incongruent with each other and unrealistically high... While work experience gained during high school was not found to be related to goal level, it is notable that high school youth with considerable work experience are less likely, all other things equal, to express educational and occupational goals that are incongruent with one another ... More able students (IQ) not only expressed higher goals, but at least among young whites were also more likely to profess congruent and realistic goals and were more optimistic about being able to achieve their goals. 45

Thus it appears that while work experience shapes work attitudes, numerous other forces have a considerable impact as well. As a final illustration of the point, while there are vast age-sex-race differences in labor market experience, differences in internal-external attitudes on the basis of age, sex, and race are virtually nonexistent.[46]

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[42] H. S. Parnes et al., Career Thresholds, I, U. S. Dept. of Labor Manpower Research Monograph No. 16 (Washington: U. S. Government Printing Office, 1970), pp. 178-85.

[43] Brito and Jusenius, op. cit., pp. 121-32.

[44] Grasso and Kohen, op. cit., pp. 28-9.

[45] Ibid, pp. 46-7.

## CONCLUSIONS

The principal purpose of this paper has been to assess the extent to which stable and successful employment careers are a function of the work attitudes and labor market knowledge of youths, rather than of traditional human capital and demographic variables most typically considered in labor market studies. A secondary purpose has been to examine what the work attitudes of youths actually are, how they differ from those of their older and presumably more mature counterparts, and how they are affected by successes and failures upon initial entry into the work force. To a considerable degree, the research summarized in this paper draws upon published works from a unique set of longitudinal data for representative national samples of 10,000 American youths and 10,000 older workers--during the second half of the 1960s --namely, the National Longitudinal Surveys.

The empirical findings reported here show clearly the importance of positive work attitudes and adequate labor market knowledge for the establishment of stable and successful employment careers. Confidence in these findings is strengthened by the fact that the observed relationships were independent of individual differences among the youths studied in a wide range of skills, abilities, and demographic characteristics, and were supported by longitudinal data as well.

The data are also persuasive in documenting the inadequacy of all types of labor market knowledge as youths cross the critical threshold from school to work--with blacks, females, and poor whites possessing the least adequate knowledge. The fact that so much of the differential in unemployment rates between youths and adults results from greater incidences of unemployment among youths, rather than longer average spells of unemployment, in itself lends support to the need for more adequate labor market knowledge for youths. Moreover, the need to adequately prepare for careers long before initial entry into the work force argues for the dissemination of more adequate labor market information years in advance of actual labor market entry.

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[46] Andrisani et al., op. cit., Chapter 4.

The role of positive work attitudes for youths in the establishment of stable and successful work careers is less straightforward and unambiguous. During the late 1960s, youths who were ambitious, self-confident, committed to the Protestant work ethic that hard work leads to success, and who liked their jobs were more likely to establish stable and successful employment careers than comparable youths who felt otherwise. Yet there is little empirical justification to consider youths' attitudes toward work as inadequate, immature (other than for lack of adequate labor market information), anti-work, or the cause of the unique labor market problems of youths--at least on the basis of the statistics for the late 1960s. The work attitudes of youths measured in the NLS data were hardly dissimilar from those reported by older workers during the same period, and there is little empirical reason to suspect that youths on the whole lacked ambition during the late 1960s. Quite the contrary, the data suggest that inadequate labor market knowledge may produce a lack of realism among youths which leads in the aggregate to overambition rather than to a lack of ambition. It may indeed be inadequate labor market information which at least in part is responsible for the relatively higher levels of job dissatisfaction, turnover, and frictional unemployment observed among youths.

The evidence is also clear and convincing in showing that unsatisfactory labor market experiences early in work careers can influence youths' attitudes in an anti-work direction, thereby reducing the chances of ever establishing a stable and successful employment career. As Glen Cain has noted:

> Conventional economists have customarily viewed 'tastes' as exogenous and as one of the (unexplored) causal variables explaining such labor market achievements as employment, wage earnings and occupational achievement. The contribution of the Dual-Radical theories lies not in reiterating the potential importance of tastes in their research, but rather in pointing out how tastes may be endogenous and a result of one's labor market achievements. Thus, the effects of discrimination, other systematic factors, or even random factors

> that can start workers off in the secondary sector (that is, in 'bad' jobs), can shape tastes in an anti-work direction and thus reinforce the disadvantaged position of low-wage workers. 47

Thus, while work attitudes among youths appear important for the establishment of stable and successful employment careers, they hardly appear to be the principal cause of the unique labor market problems of youths. It seems highly unlikely that secular changes in the work attitudes and labor market knowledge of youths since the late 1960s could have had nearly as much to do with the worsening youth labor market situation of this decade as have such factors as changes in the number of youths looking for work and changes in the aggregate demand for labor. While it is unclear how much a part the work attitudes of today's youths have to do with the worsening youth labor market situation, it seems entirely clear what effect the loose labor markets of the 1970s have had on youths' attitudes toward work. Loose labor markets lead to a deterioration of positive work attitudes among youths which makes it all the more difficult to establish stable and successful employment careers. The loose labor markets of the 1970s can be expected to have had such an effect.

The implications for public policy are essentially twofold. First, increase the availability of all types of labor market information early in the life cycle, particularly for young blacks, young women, and poor white youths. Second, employment and training programs could seek to offset the negative, anti-work effects of loose labor markets and the pathology of the youth labor market on the work attitudes of youths. Long run incentives to work, and giving good reason for youths to believe that hard work and human capital investments now will eventually pay off in the labor market are clearly needed. The anti-work effects of the loose labor markets in the 1970s on youths' attitudes can be particularly important for many years to come. After all, the anti-work effects are being felt

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47. G. G. Cain, "The Challenge of Dual and Radical Theories of the Labor Market to Orthodox Theory", paper presented at the American Economic Association Annual Meetings, San Francisco, December, 1975, pp. 18-9, emphasis in original. (mineographed).

by one of the largest cohorts of youths to ever enter the labor market. In addition, this cohort of baby boom youngsters faces what may possibly be, relative to their aspirations, perhaps the worst set of labor market constraints ever to have faced preceding generations of youths.

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# ECONOMIC AND SOCIOCULTURAL VARIABLES AFFECTING RATES OF YOUTH UNEMPLOYMENT, DELINQUENCY AND CRIME

By: Daniel Glaser

## ABSTRACT

Research has repeatedly found a direct correlation between unemployment rates and crime rates, but not so powerful and consistent a relationship as to exclude other factors in crime causation. This paper will first survey social-science literature on economic variables affecting law-violation rates. It will then present briefly some evidence and inference that social, cultural and political aspects of the lives of many of today's youths are major determinants of both their unemployment and their offenses.

## INTRODUCTION

The simple theory that crime is a means of obtaining wealth or of expressing anger when legitimate economic pursuits fail has probably been propounded since ancient times, and has been tested statistically at least since the early nineteenth century (Vold, 1958: Chapter 9). Four distinct methodologies differentiate these studies, which can be categorized by the types of data that they employ, as: human ecology, social class, business cycle, and circumstances of the offender. This research was always handicapped by deficient information, especially on crime, and probably by failure to investigate adequately the qualifications that the simple theory requires to fit reality. Nevertheless, even a cursory review reveals that findings from each of these four types of investigations, while somewhat inconsistent and controversial, predominantly show that extreme poverty (in comparison to the rest of the community) is highly correlated with crime.

## ECOLOGICAL STUDIES

A "social survey" movement spread from one city to another during the nineteenth and early twentieth centuries. Perhaps the most influential of its publications in the English-speaking world was Charles Booth's Life and Labor of the People of London, issued piecemeal from 1886 to 1903.

The movement's many similar studies illustrated, for a large number of metropolises, the concentration of both crime and poverty in the same neighborhoods. Somewhat more objective evidence was provided by Clifford Shaw and Henry D. McKay (1929, 1942). Their maps and tables showed how delinquency rates varied in Chicago's census tracts. For comparison they made similar maps and tables for such indices of poverty as average rent per residential unit, average number of occupants per room, and infant mortality rates. Thus these publications demonstrated, but did not measure, the extent to which delinquency was concentrated in areas with peak rates of poverty, disease, divorce and all other indices of what they called "social disorganization." Their 1942 book also reported similar studies that their work had inspired in other American cities, all of which had analogous findings.

For two decades following World War II, the analysis of census tract rates for delinquency and other variables--such as income, rent, ethnicity, housing, crowdedness, and every other item that the census tabulates by these tracts--was made more rigorous in several cities through multivariate statistical computations. Gordon's (1967) thorough review of this research, however, summarizes:

> All of these studies misused such ... procedures as partial correlation, multiple regression and factor analysis. In addition, these studies ... have been affected by serious artifacts stemming from the accepted practice of using indexes with mixed cutting points, some of which are more sensitive to the tails of their distributions than others. When all of these errors are taken into account, it turns out that the association between delinquency and socioeconomic status is quite unambiguously very strong.

He adds an extremely astute message for policymakers:

It is the extremely low end of the SES range that is most relevant.

> This finding ... contains a warning concerning the conduct of antipoverty programs. It suggests that, in order to decrease delinquency, for example, it is necessary to reach the very bottom-most stratum in every census tract. Simply pumping money into low-income areas may result in helping needy people, but they may not be the ones chiefly responsible for the high social pathology indexes from which intervention against poverty now derives its main political justification. To the extent that programs fail to reach this lowest stratum--however successful they are at assisting the more accessible higher-stratum poor--they will fail

> to alleviate the more intractable and socially visible consequences of poverty. Certainly there is much to be said, on humanitarian grounds alone, for directing limited resources toward the people best able to take advantage of them. ... Nonetheless, there remains the possibility that the failure of programs to materially reduce delinquency and eliminate hard-core poverty will trigger political reactions that make it impossible to gain support for efforts that would benefit the very poorest. For these people's own misery to be used to legitimate help for someone else, and in a manner that diminishes their own chances of eventually receiving help themselves, would be the ultimate exploitation.

Delinquency, as a legal concept, refers only to offenses committed by juveniles. While defined somewhat diversely in our fifty states, it usually encompasses all acts by persons below a particular age--usually eighteen--that would be prosecutable as crimes if the perpetrators were older. It also includes acts for which only juveniles can be placed in state custody. These so-called juveniles status offenses are mainly truancy from home or school, and incorrigibility (defined as persistent or flagrant disobedience of parents, teachers or ther adult authorities). Adult crime, however, includes many offenses by youths, since the median age of arrest for major crimes (the FBI's "Index Offenses") was 19.2 during 1976 (a puzzling increase of about one year over the median in preceding years), and about one-third of the arrestees were between eighteen-and-twenty-five-years-old in every recent year.

There has been much less ecological research on the neighborhood correlates of adult crime rates than on delinquency. The major study was Schmid's (1960) factor analysis of a 38 x 38 correlation matrix of twenty crime indexes and eighteen economic, demographic and social variables from the 1950 census for ninety-three census tracts of Seattle. By far the highest correlations in the matrix were between the percentage of males unemployed and the rates of various specific offenses, notably .82 for drunkenness, .83 for fighting, .85 for other disorderly conduct, .82 for lewdness, .85 for petty larceny, .80 for theft from automobile, and .85 for highway and car robbery. Correlations of median income with the same offenses ranged from .48 for lewdness to .58 for theft from automobile. It should be noted that the location of arrests for drunkenness, disorderly conduct and lewdness often tells as much about neighborhood variations in police practices as about variations in citizen behavior.

Schmid's (1960) factor analysis identified eight clusters of intercorrelated variables. The first factor, with by far the highest loadings, he labeled "Low Social Cohesion--Low Family Status." It revealed strong correlation of social conditions characteristic of slum rooming house areas near business districts--low proportion of homes owner-occupied, high percentage of labor force unemployed, low education levels, low percentage of adults married, high proportion of females in the labor force and low status occupations--with such crimes as automobile theft, theft from automobiles and shoplifting. The second factor, which was labeled "Low Social Cohesion--Low Occupational Status," identified by high factor scores the poorest residential neighborhoods, contiguous to Skid Row and mostly occupied then by blacks and Orientals, but by low scores it also identified the better residential areas with mainly white collar occupations or higher blue collar jobs. The demographic components of this factor were low status occupations and low education levels, but a high proportion married, high fertility rates, few of the females in the labor force and high proportion of housing over thirty years old; it was linked to the same types of crime as Factor 1. This study's extremely high correlations of crime with unemployment and its two strongest factors emphasize the linkage of crime to the poorest of the poor neighborhoods, much as did the multivariate analysis of delinquency rates.

A different ecological research method divided cities into "social areas" by three composite indicators of census tract characteristics: Economic Status, determined by prestigious occupations and high levels of education; Family Status, shown by high fertility rates, low percentage of women working, and high percentage of single-family dwellings; Ethnic Status, ascribed to areas with a high percentage of native-born whites (Shevky and Bell, 1955). Applying this scheme to Lexington, Kentucky in 1960, Quinney (1964) found both crime and delinquency rates inversely related to economic and family status but positively related to ethnic status. Further analysis showed, however, that the ethnic status finding occurred because offense rates were highest for nonwhites living in areas occupied predominantly by whites and highest for whites in areas with the largest proportion of nonwhite residents. Furthermore, high family status prevented crime only in areas of low economic status.

A quite contrasting type of ecological analysis uses total cities or metropolitan areas as units, rather than census tracts within these communities. Schuessler and Slatin (1964) factor analyzed 1950 and 1960 crime and census variables for cities of over 100,000 population in the United States. In both periods the factor most associated with crimes against the person, which they first called "Social Frustration" but relabeled "Minority Relations," had major loadings from high percentage nonwhite, high housing crowdedness, and low percentage of young people in school. The factor most associated with property crime, which they labeled "Anomie," loaded high on only one census variable in 1960, percentage divorced, but in 1950 was also identified by low percentage of population engaged in manufacturing, low percentage of population in families, low median schooling of males over twenty-four-years-old, and low median monthly rent. Economic variables were clearly far from dominant in the conditions that this study linked with city crime rates.

Eberts and Schwirian (1968) hypothesized that crime would be evoked not by a particular low level of wealth or income in a city, but by the relative deprivation of some persons there as compared to the rest; essentially, they ascribed crime more to inequality than to poverty. Using data on 200 Standard Metropolitan Statistical Areas from the 1960 census and FBI figues on offenses known to the police, they found crime rates highest in metropolises with the greatest disparity between the proportion of the population with annual income over $10,000 and the proportion with income under $3,000. Thus crime rates were higher where there was a large upper class or a large lower class than where these extreme strata were nearly equal in size. This finding was clearest in the biggest metropolitan areas, in those with a high percentage of nonwhites, and in the South; outside the South, crime rates were especially linked to a high proportion in the upper class rather than to a more balanced income distribution or the largest proportion in the poorest class.

Eberts and Schwirian also classified metropolitan areas by the percentages of their white and nonwhite populations having white-collar jobs, compared to national percentages for each of these racial groups. Crime rates were largest in metropolitan areas where these percentages were highest for whites and lowest for nonwhites; it was the disparity in availability of white collar employment more than the actual proportion of the white or nonwhite proportion receiving it that was most related to offense rates. This finding applied to both large and small metropolises, and to the non-South as well as the South, although it was most pronounced in areas with a low percentage of the population nonwhite. Thus both of their analyses confirmed that inequality is the economic variable most correlated with high crime rates. It would be interesting if such analyses were done separately for crimes by youth, and for different types of offenses.

A still different type of ecological analysis uses states within our nation as its units, and its findings have been of major interest primarily for homicide. Many criminology textbooks have for decades suggested that regional cultures are major determinants of crime rates in the United States, pointing out that most offense rates of states increased as one moved from the Northeastern to the Far Western states, but that the Southeastern states combined by far the highest homicide and assault rates in the nation with low rates of crimes against property. Hackney (1969) and Gastil (1971) extended the analysis of Southern homicide rates. They emphasized (1) the slavery and absence of industry in Southern history, (2) the traditions of dueling and vigilantism, and (3) statistics that showed higher correlations between state homicide rates and whether a state had been in the Confederacy during the Civil War than the zero-order or multiple correlations of the homicide rates with median income, education or a host of other economic and social variables.

Loftin and Hill (1974), however, present impressive evidence that it is not a state's Southernness but its degree of extreme poverty or of poverty-conducive conditions that is most predictive of its homicide rate. Thus the zero-order correlation of state homicide rates in 1960 was -.43 with median income but .67 with the percentage of a state's families having annual income under $1,000, .77 with the percentage of the population illiterate, .82 with the percentage of the population over twenty-four-years-old with less than five years of education, .83 with the percentage of its inductees failing the Armed Forces Mental Test, .85 with its infant mortality rate, .88 with the percentage of its children living with only one parent and .93 with a "Structural Poverty Index" based on all of the foregoing except median income. The correlation of homicide rates was .80 with a state's being in the Confederacy during the Civil War and .87 with Gastil's (1971) index of the Southernness of each state's population (an index for each state, which for non-Southern states considers how many migrants it received from the South and other evidence of probable Southern cultural influence). Incidentally, state homicide rates correlated .85 with percentage of the state population nonwhite, and the Structural Poverty Index correlated .84 with Southernness. In two multiple correlations of state homicide rates with diverse independent variables, one with a dummy variable for Confederacy and one with the Southernness Index, Structural Poverty had by far the highest Beta Weights.

Thus, whether the units of ecological analysis are neighborhoods, cities or states, both zero-order and multiple correlations between rates of delinquency or crime and indices of extreme poverty are quite high. Nevertheless, persons who are skeptical that poverty is a primary cause of crime point to rich people who violate the law and poor people who do not. Specifically, critics of ecological studies point to law-abiding individuals in high offense-rate neighborhoods, and to some neighborhoods with much poverty but low offense rates. Such exceptions are among the reasons why statistical correlation coefficients are less than 1.0, but they may also be clues to factors that counteract poverty in crime causation.

Quinney's (1964) Lexington study, already cited, was one of many to find that cohesive family bonds distinguish the nonoffenders in high delinquency areas. Similar conclusions were reached by the Gluecks (1950) in their landmark comparison of delinquents and nondelinquents from the same Boston neighborhoods, and also by more recent studies. Hirschi (1969) provides persuasive data on family and nondelinquent peer friendship bonds acting as "controls" against temptations to delinquency to which, he suggests, most of us would otherwise succumb. Sociologists have also repeatedly demonstrated by illustration--not by adequate statistics--that when low offense rates characterize very poor neighborhoods or towns, the residents generally are of the same ethnicity, have cohesive extended families, and participate actively in local religious or other organizations. Examples in the United States included Chinatowns and Japanese-American neighborhoods that formerly were both extremely poor and relatively free of crime. More recent examples are sections of African cities settled by a single tribe as compared with mixed tribal neighborhoods (Clinard and Abbott, 1973, 1976; Glaser, 1978: 224-25). Yet, interethnic mixture is inevitable in modern cities; although the weakening of ethnic institutions and a resulting increase in crime are often its temporary consequences, its long run effects probably are to reduce both inequality and crime. However, the most influential assertions that economic conditions cause crime focus on conflict among groups with opposing interests in the means of production, but these assertions have been a source of discord in the academic community.

## SOCIO-ECONOMIC CLASS AND YOUTH CRIME

Karl Marx wrote little on crime, but designated all private property as "theft." He was unsympathetic to criminals, whom he called "scum" when warning his followers against alliances with them (Hirst, 1975). However, early Marxist criminologists, notably Holland's Willem Bonger (1905/1969), ascribed crime to poverty and blamed poverty on capitalism. This theme is widely asserted in Britain and the United States by self-styled "radical" or "critical" criminologists. They proclaim that we could "rid society of thievery by abolishing the precondition for theft--namely private property"

(Taylor et al., 1973:174), ignoring the theft of public property in all societies, and that the most serious crimes everywhere are against persons, including theft of their legal rights.

The ecological data already cited, and statistics on the income or occupation of arrestees or convicts and their parents, provide ample and consistent evidence for those who claim that inequality of socio-economic status causes crime. Critics of all political persuasions, however, question the adequacy of official statistics that link crime to the lower classes on two major grounds. The first is that crimes of the upper classes, such as monopolistic practices, misrepresentations in selling, tax evasion and other so-called white-collar offenses are not greatly prosecuted, although their dollar costs are always estimated as totalling many times that of ordinary property crimes, such as theft, burglary and robbery. The second is that ordinary offenses are also committed by the middle and upper classes, but police and courts enforce the law disproportionately against low-status offenders. A third, albeit less major, criticism by some criminologists, is that all violations of human rights are criminal, and therefore, any act fostering poverty, racism, sexism or war is a crime (Schwendingers, 1970). There are good methodological arguments for defining "crime" more narrowly, yet in its broadest legal sense, as any act for which a court may lawfully impose punishment; but criminologists should still be concerned with how economic elites and other pressure groups tend to determine the content of laws that specify what courts may punish (Glaser, 1978: Ch.2).

Ostensibly compelling evidence that delinquency rates are not related to socio-economic class was provided by questionnaires given to junior high and high school students, who were asked to check which of a list of offenses they had committed. Usually these forms were anonymous--the students' names were not required, but later research shows that requesting names makes little difference in the responses. The students were also asked to indicate the occupations of their parents, and the surprising finding was that students from every social class grouping of parental occupations reported delinquency at about the same rates (Nye et al., 1958; Dentler and Monroe, 1961; Akers, 1964; Erickson and Empey, 1965; Hirschi, 1969). Diverse checks, including use of a lie detector, have shown these questionnaire admissions of law-breaking to be quite valid (see Clark and Tifft, 1966).

There are two major methodological detects in the above procedures. In the first place, all these studies either were done at only one school or pooled the responses from different schools. It is possible that students within any school develop similar standards of conduct regardless of the occupations of their parents--the school is a subcultural unit--but that schools from neighborhoods of very contrasting economic status differ in the delinquency of their juvenile subcultures. It is also possible that the classification of parental occupations, usually by census categories, is misleading as an index of socio-economic class when different types of neighborhoods are pooled. For example, most business proprietors or salespersons living in a slum may have much less income than persons with the same occupation who live in a wealthy suburb; the slum residents may average as little as $10,000 per year and the suburbanites over $100,000.

The above hypotheses seemed to be validated by Clark and Wenninger (1962), who administered questionnaires in a poor rural school district, an inner city metropolitan ghetto, an extremely wealthy suburb, and a small industrial city. There was no marked relationship within any of these districts between father's occupation and admitted delinquency, but on the whole, offense rates were lowest in the rural area and highest in the slum ghetto. The slum juveniles reported more theft, violence, truancy from school, deliberate disruption of school activities and vandalism than did youngsters elsewhere. The suburban students reported more drinking, gambling and use of pornographic literature, offenses probably reflecting their affluence, as well as a fair amount of truancy. Hardt (1968) reported similar findings in comparison of lower and middle class neighborhoods of another metropolitan area. It should be stressed, however, that the contrasts between neighborhood admitted-delinquency rates reported in these studies were not nearly as great as those indicated by police records. Hardt procured arrest records for the areas in which he gave questionnaires and he also asked the students whether they had been ticketed or arrested by the police. The results showed that police are much more inclined to arrest in poor than in middle class areas. Several case studies of middle and upper class delinquency (e.g., Chambliss, 1973) also indicate the ease with which some affluent youth can "get away with" crime.

A second source of error in these studies is that the most delinquent students are likely to be truants, hence absent when the questionnaires are distributed. This biases the research to underreport delinquency. Such an error was demonstrated through a study in London, by the Canadian sociologist Lynn McDonald (1969), who found neighborhood differences in admitted delinquency analagous to those reported by Clark and Wenninger and by Hardt. When she traced and contacted most of the students who were absent when she administered her questionnaires to the schools, she found that they had higher crime rates than those who were present, and that absenteeism was highest in the low status areas.

An additional defect not inherent in the above type of methodology characterized most of the early studies. In order to have offenses that scaled statistically, so that they could ask about many offenses but assign a single delinquency score to each student, the researchers included inquiries on very petty infractions, such as speeding, and deliberately disobeying parents. Some also failed to ask about the frequency of offenses. Virtually all studies that provide separate data on a wide range of offenses and by frequency, even within school districts, find that children of low status parents differ from those of higher status in reporting that they more often used violence and stole items of appreciable value (see also, Gold, 1970).

These admitted offense questionnaires have been used almost exclusively with students. In their one major application to adults (Wallerstein and Wyle, 1947), an amazing volume of serious offenses were admitted, but the study's only statement on the relationship of crime rates to status was that: "The mean number of offenses committed in adult life (over age 16) for men, classified according to occupation, ranged from 8.2 for ministers to 20.2 for laborers, with a mean of 18 for all men."

During 1973 the U.S. Bureau of the Census conducted a national sample survey for the Justice Department in which persons were asked if they had suffered any of a list of major crimes in the previous year, in addition to questions on their social and economic characteristics. Those who reported the lowest annual incomes most often reported victimization from assaults and robberies from the person, but the poorest and the wealthiest were highest in household burglaries (Law Enforcement Assistance Administration, 1975:18, 22). Since all of these crimes except burglary of affluent homes are usually ascribed to youths in the area and since a neighborhood's residents tend to be of similar economic status, one can infer from the victim's reports that such crimes are committed most often by the poorest youths. The relationships of victimization rates to income were similar for blacks and whites.

In summary, the traditional conclusion that the lowest socioeconomic classes have delinquency and crime rates greater than those of the upper classes is predominantly supported by questionnaire studies, but less strongly than by arrest rates. Therefore, it is erroneous to assert that all socio-economic classes have the same law-violation rates; most types of ordinary crimes are demonstrably perpetrated more often by poor than by affluent youths. Yet so-called white-collar offenses certainly are committed most often by adults in high-status occupations, since they have the most opportunity to engage in such crimes.

## CRIME AND THE BUSINESS CYCLE

Efforts to relate fluctuations in the frequency of crime to changes in various economic indicators, from unemployment rates to the price of rye, were undertaken during most of the nineteenth century in Europe, and were extended to the United States during the twentieth century. The results were highly inconsistent and controversial. After surveying these undertakings, Vold (1958: 164-81) concludes that: "...assumptions involving either positive or negative relationships with economic conditions may be supported with some show of statistical significance. The obvious inference is that the general relations of economic conditions and criminality are so indefinite that no clear or definite conclusion can be drawn."

Despite the pessimism that this earlier research evoked, studies during the past twenty years have quite consistently related variations in crime rates to shifts in the extent of unemployment, especially when they also considered interacting variables. Glaser and Rice (1959), controlling only for age, found marked positive correlations between arrest rates of males twenty-one-year-old or older in Boston, Cincinnati and Chicago during 1930-1956 and U.S. national Age-Specific Male Labor Force Unemployment rates, but negative correlations for persons under twenty-one. The positive correlations of crime with unemployment for men twenty-one and over were especially marked and consistent for theft in all three cities, but only in Boston and Cincinnati for crimes against persons and for misdemeanors: Chicago had a variable pattern on these two types of offenses. The inverse correlation of arrest rates with unemployment for males under twenty-one was consistent in all three cities only for crimes against persons and for misdemeanors, but not for the positive correlations for men twenty-one and over.

The Glaser-Rice study focused on these three cities because they were the only places for which we could find age-specific arrest rates compiled in a uniform manner for a long range of years. The FBI's age-specific national arrest totals had a highly variable population base, for many years came only from cities that comprised under half the United States population, and reflect drastic changes in methods of collecting arrest information, especially after 1952; thus we had no way of knowing for any long range of years the population base from which to convert the FBI arrest data to rates. Nevertheless, we calculated each age group's proportion of total arrests from the FBI figures for 1932-1950 and correlated these proportions with the percentages of U.S. Labor Force Unemployed in these years. The correlations were consistently positive for ages twenty-one through thirty-four for property crimes, for crimes against persons, and for misdemeanors; they were consistently negative for arrestees eighteen and under for property crimes and for crimes against persons, but they were also negative for all three categories of crimes for persons thirty-five and over, and were inconsistent for other age and offense groups. It is possible that the negative correlations for those thirty-five and over are artifacts of using proportions of total arrests instead of rates for each age group, since this procedure makes figures for the various age categories interdependent. Incidentally, similar results were obtained using total and age-specific unemployment rates.

In planning this research I wished to control for age because I presumed that unemployment would most strongly affect crime rates for men over twenty-one, most of whom are in the labor force. I hypothesized that juvenile offenses, conversely, might increase in periods of full employment, because then more mothers enter the labor force and there is increased use of commercial recreation that separates parents from children. The finding that positive correlation with unemployment for men over twenty-one was strongest and most consistent for property crimes (since these are illegal substitutes for jobs), while negative correlations for juveniles were more marked and consistent for crimes against persons and for misdemeanors (which are mostly either violent or disorderly acts), seemed to support the theory underlying these hypotheses.

Gibbs (1966), however, offers an alternative explanation for our findings. He interprets our data as support for his theory of status integration, with which he has analyzed various forms of deviant conduct and usually has procured positive but also debatable results (see Li, 1971). Essentially, the theory asserts that conduct will be most deviant under conditions that are most unusual for persons in a given status, and unemployment is least unusual in our society for men over twenty-one and under retirement age. Incidentally, inspection of the raw data indicated that very high juvenile arrest rates accompanied full employment of adults only during the World War II and immediate postwar years, when numerous adult males were away in the armed forces and many families were relocated far from kin, leaving adolescents especially on their own.

More recent research relating crime to unemployment has been done by economists. They used much more sophisticated statistical methods than were previously applied to this task. The major study is that of Belton Fleisher (1966), who limited himself to offenses by persons under twenty-four-years-old. He used multiple-regression analysis, adding many variables to the unemployment and arrest data, including: (1) the number of men in the armed forces, to take into account the effects of war mobilization on family life; (2) what he calls "taste" variables, namely, the proportion of women over fourteen-years-old who are separated or divorced, the proportion of residences that are owner-occupied, and the median years of schooling of the adult population; (3) dummy variables for region and for years in which there were changes in the FBI's method of collecting age-specific arrest data; (4) additional variables for social conditions, notably percentage nonwhites and geographic mobility of the population. He applies these variables to relating unemployment and crime either longitudinally over time or cross-sectionally at one time, to 101 U.S. cities and seventy-four Chicago census tracts plus forty-five Chicago suburbs, and also presents some British data. His time-series analyses show marked positive correlations, especially for those over seventeen, but a negative correlation for those under seventeen in England and Wales. His cross-sectional analysis, which is really an ecological study, finds youth arrests in high arrest rate areas more a function of mean income than of unemployment rate. He estimates that in these areas "a ten per cent rise in income might well result in a twenty per cent decline in delinquency." Although one dare not extrapolate that a fifty percent increase in income would eliminate delinquency, there is much to be learned from his data and his cogent commentary.

Phillips, Votey and Maxwell (1972a, 1972b) extend Fleisher's analysis somewhat by: (1) considering as separate independent variables related to unemployment (a) the race-specific proportion of an age group in the labor force (which has declined for youth as more go to college, but also as more give up searching when long unable to get jobs), (b) race-specific rates of unemployment, and (c) the fraction of eighteen- and nineteen-year-old males of each race in the noninstitutionalized population (which changes mainly from shifts in the size of our armed forces); (2) considering as separate dependent variables the four most frequent property crimes--larceny, burglary, robbery and auto theft--

but only for eighteen-and nineteen-year-olds. Because of collinearity, the independent variables predicted crime rates better if racial categories were combined and all the population was categorized by either of two trichotomies: (1) working, nonworking (either unemployed or not in the labor force) and other; (2) in the labor force, not in the labor force and other. Variations in the values for each of these two trichotomies predicted offense rates equally well. Age-specific offense rates were estimated for youth by dividing the proportion of all arrests in the eighteen-to-nineteen-year-old age category by the ratio of clearances by arrest to total reported offenses; this assumes that there is the same age distribution in nonarrested as in arrested offenders. These procedures permitted remarkably accurate estimation of youth offense rates from the employment data. They conclude that the increases in youth crime rates during the 1960s did not indicate that youths were becoming more criminal but rather, that the economic difficulties conducive to their committing crimes were increasing (see also, Votey and Phillips, 1974). Several other multivariate regressions of crime with unemployment have been published (e.g., Ehrlich, 1973; Greenberg, 1978), but none, I believe, that focus on youth crime.

Discussion of crime and the business cycle should not be concluded without considering some rather remarkable fluctuations in the offense rates of metropolitan youths that occurred in the late 1960s and early 1970s which, as far as I know, have not been analyzed by economists. Nationally, homicide rates increased from 9.6 to 18.4 per 100,000 in cities of over a million population between 1965-1970, and reached 24.6 in 1975, but declined slightly thereafter. This was mainly a metropolitan phenomenon; the national homicide rate changed from 5.5 in 1965 to 8.3 in 1970 to 10.2 in 1975, and all city categories of less than a quarter-million had increases less pronounced than the surge in national totals. The rise in robbery rates reported to the FBI was even more pronounced from 1965 to 1970 in cities of over a million, increasing from 221 per 1000,000 in 1965 to 778 in 1970, then only to 879 in 1975, and declining thereafter; the national robbery rate went from 72 in 1965 to 172 in 1970 to 218 in 1975.

There are no published national tabulations to indicate the contribution of different age groups in large cities to such increases in these two types of violent crime. Special studies for Chicago, however, probably indicate developments in most metropolises of the United States in this period, for the social and cultural groupings and sentiments were similar in many cities. Between 1965 and 1970 the rate of homicides noted by the Chicago police more than doubled, but homicide arrests of black males fifteen-to twenty-four-years-old almost tripled and homicide victimization rates for this population category more than tripled. During this period the percentage of homicides that occurred in the course of robberies rose from eight to eighteen in Chicago but the number robbery killings ascribed to offenders fifteen-to twenty-four-years-old increased more than sevenfold, with both arrestees and victims in these offenses disproportionately black. There was also a 791% increase in the number of homicides ascribed to groups of black males, aged fifteen to twenty-four, as compared with 136% increase in the homicides alleged to have been done by lone offenders in this population category. Finally, the proportion of homicides ascribed to black males aged fifteen to twenty-four involving the use of firearms increased 444% between 1965 and 1970, compared to 69% for their homicides by all other means (Block and Zimring, 1973). These trends continued through 1973, but were less pronounced after 1970 (Block, 1975).

The distinctive feature of the early 1960s in the history of the United States was the progress of the civil rights movement. This greatly increased expectations of increased equality of opportunities among black youths. The lag in realization of these hopes, especially in the metropolitan ghettos, is alleged to have been the main factor in precipitating a series of riots in these locales, beginning with the Watts riot in Los Angeles in 1965. These disturbances intensified following the assassinations of Martin Luther King and Robert F. Kennedy in 1968, but were largely terminated in the early 1970s.

From 1965 on, the mass media vividly portrayed this violence, bringing it pictorially into everyone's living room. This furthered both black youth violence and white backlash. More than ever before, the nation became an armed camp, with manufacture and importation of handguns rising from one million in 1965 to over three million in 1969, then declining to about two million annually by the mid-1970s (Newton and Zimring, 1969:174; U.S. Department of Commerce, 1976:156).

To claim a full understanding of this upsurge in violent crime would be presumptuous, but it seems reasonable to infer that it was in large part a consequence of a growing sense of relative deprivation among ghetto youths. Their frustration at the inaccessibility of legitimate ways to realize the "American dream" that Dr. King had portrayed evoked an anger that usually was directed at the most convenient targets, other blacks. But the wave of ghetto riots were followed by some reduction of both perceived and actual inequality--social, political, and economic--sufficient to facilitate further progress by nonviolent means. Although social scientists debate how much this progress has slowed down in the 1970s, it does not appear to have stopped; black youth increasingly have seen prospects of improving their lot by education, and have gained much white collar employment, despite the fact that an academic degree may be less of a guarantee of a good job than it was a few years ago (Hauser and Featherman, 1976; Taussig and Danziger, 1976; Farley, 1977). It would be interesting to determine whether there was decreased predictability of black youth crime rates from employment data during the 1970s, and whether youth in the Latino barrios of our metropolitan areas are beginning an upsurge similar to that of the black youth a decade earlier, for somewhat comparable reasons.

## EMPLOYMENT STATUS OF OFFENDERS AT THE TIME OF THEIR CRIMES

A fourth method of investigating the economic factor in law violation has been to try to determine the rate and quality of employment of offenders at the time they commit their crimes. Such inquiries are most easily pursued when research subjects are under some type of government control. Methods used include (1) interviewing convicts in prison about their employment when they committed their crimes, (2) correlating the employment information routinely collected on probationers and parolees while they are under supervision, with their subsequent recidivism rate, and (3) interviewing them regarding their employment when they make their required regular visits to the supervision office.

All the above procedures were used in Glaser's (1964) study of federal prisoners and parolees. The parolee interviews were replicated and extended by Pownall (1969). Only a quarter of the federal prisoners were found to have been employed during 75% or more of their last two years in the community, 38% were employed 25% to 74% of this time, 27% worked less than a quarter of this period and 6% not at all, while 4% were students throughout these last two years out. Postrelease employment and earning rates were inversely related to recidivism rates on parole. Also, in almost all probation and parole prediction studies testing this variable, prior employment is inversely related to recidivism.

A 1974 Bureau of the Census survey of inmates in state prisons, conducted for the Department of Justice, concluded from responses of the prisoners that almost half had worked for twenty-eight weeks or less on their last job, 42% had annual incomes near or below the $2,492 then designated by the government as the poverty level, and their employment was disproportionately in unskilled labor, operative and service jobs (U.S. Department of Justice, 1976:25). A Rand Corporation study probed in depth the official and actual crime and work careers of forty-nine California prisoners convicted of robbery who had at least one prior prison term. The study found that only about half had depended on a regular job as their usual source of income, that 10% never were interested in a regular job, and that only about 15% claimed that loss of a job had contributed to their committing their last offense. Most of their jobs were of poor quality, but those with the best employment records were the most intermittent in their offenses (Petersilia et al., 1977). A Rand survey that contacted about 1400 inmates who offered a fairly representative sample of California prisoners, found from a factor analysis of their responses to probings on the motivation of their crimes, that about equal weight was given by them to economic duress (e.g., losing a job, heavy debts) and "high times" (e.g., to get money for drugs or alcohol, excitement and kicks). Those with the most stable employment had the least involvement in crime (Stambul and Peterson, 1977).

That destitution evokes crime is suggested also by experiments in California and Maryland which demonstrated that recidivism of parolees is reduced if they are paid modest sums--$60 to $80 per week for up to 12 or 13 weeks--when unemployed (Reinarman and Miller, 1975; U.S. Department of Labor, 1977). These programs were justified, in part, by the fact that eligibility for unemployment insurance requires a minimum amount of insurance-covered employment during the preceding 12 to 18 months (amount and duration depend on state laws), but a person confined in this period (e.g., as an inmate of a prison or a mental hospital) cannot have such employment. I understand that repetitions of these experiments in Georgia and Texas did not result in recidivism reduction, but reports on these studies are not yet available. However, California in 1977 enacted S.B.244 which provides credit for work in prison toward payments when unemployed on parole.

Studies of offenders' circumstances, like the other three types of inquiry into economics and crime reviewed here, leave the impression that extreme failure at legitimate employment is highly correlated with serious law violations. Yet this correlation probably is imperfect not only because of measurement difficulties, but (1) because of variations in the way people assess their economic deprivation, and (2) because of other social and cultural influences inhibiting or fostering involvement in crime. Some of the latter influences may affect not only crime, but also prospects of employment.

## SOCIOCULTURAL AND POLITICAL FACTORS IN TODAY'S YOUTH CRIME

Any study of youth unemployment and youth crime should analyze the history of these problems. A view of the past yields somewhat different conclusions for different types of offense (elaborated in Glaser, 1978), but suggests that for most youth crimes in the United States today three developments are especially influential: (1) social separation of youths from adults, (2) differentiation of students in elementary and secondary school experience, and (3) futile criminalization of some types of drug use. These trends, it can be argued, not only supplement unemployment in causing youth crimes but also cause much youth unemployability.

## Segregation of Youth

Formerly, the activities of adults and children were much less separate than they have now become. The rapid increase in ownership of home appliances, automatic central heating, no-iron clothing and pre-processed food has steadily diminished the time required for household chores and the extent to which they are shared by children and parents. The replacement of small family businesses by corporate enterprises has eliminated what once were intensely collective interests of the parents and all their offspring old enough to help; family businesses have shrunk from the major form of employment to a relatively small role in our economy, especially as the proportion of the population engaged in farming has declined.

The time that youth spend in school has grown continuously. The median level of education of our population over twenty-four-years-old rose from 8.6 years in 1940 to 12.1 in 1970. Contrastingly, until the beginning of the twentieth century a majority did not even enter high school and until about 1950 more entrants dropped out than graduated. The academic year has been lengthened by more than a third during the past half-century. The amount of time that students spend away from home per school day and on weekends has also grown because of burgeoning extra-curricular activities in school and commercial recreational activities catering primarily to youth. These developments, plus more employment of both parents away from home, have steadily expanded the proportion of their waking hours in which youth are in close personal contact only with persons of about their own age (Gillis, 1974; U.S. President's Science Advisory Panel on Youth, 1974; Glaser, 1978:Chapter 8).

A basic law of sociology and anthropology is that social separation produces cultural differentiation. Because of their increased separation, today's youth probably diverge from their parents in tastes and values more than did youth of any former era. This contrast between the generations is readily evident in their music and dancing preferences, and it was demonstrated statistically by the 1977 Gallup Poll findings that most Americans eighteen to thirty years old had smoked marijuana, but only 5% of those over fifty had tried it. Such generational differences probably can be reduced only by increased collaboration of juveniles with adults in common interests and activities. Unless the age segregation in our society diminishes, not only will the generations disagree on marijuana, but many youths will be especially deficient in qualifications for successful employment in today's industrialized and urbanized society. These handicaps, however, do not divide the generations as much as they differentiate the youth population, and they result from conditions in our schools.

## Differentiation of Students

Because the years spent in school and the schooldays per year have grown while time spent by youths with their parents has diminished, the school's potential influence on the younger generation probably has increased. But those who become seriously retarded in basic learning skills in the early grades find their classes in subsequent years especially frustrating, humiliating and boring. All youths seem to seek respect from peers, and those who cannot get it by academic work search for it in alternative types of activities, including delinquency. More than ever before, research finds that dislike of school, poor grades, and classroom misconduct are predictive of later law violations, both when offenses are measured by infractions that youths admit on questionnaires or by their arrest records (Hirschi, 1969; Empey and Lubeck, 1971; Polk and Schafer, 1972; Frease, 1973). Multivariate analysis indicates that a poor school record has become more closely related to delinquency than belonging to a lower socio-economic class or a minority group (Polk and Halferty, 1966; Jensen, 1976). When juveniles acquire delinquency records while in school, however, their rate of further offenses diminishes if they drop out, and declines even more if they also marry, get jobs, or both; thus delinquency becomes less persistent if they leave the isolated social and cultural world of adolescents maladjusted to the student role and acquire legitimate roles in the adult world (Elliott and Voss, 1974).

Contrastingly, students who like school and are fairly successful in meeting its academic requirements are thereby prepared for legitimate adult roles. They usually are less separated and alienated from adults than students doing poorly in school, and they are much less likely to acquire a criminal record. Engaging in extracurricular activities is also inversely correlated with law violation (Hirschi, 1969), and such activities other than athletics are especially associated with later mobility in adult occupations (Spady, 1970; Otto, 1976).

It is probable that both classroom and many extracurricular groups prepare youths not just with substantive knowledge needed for adult occupations, but with a complex of lifestyle and communication habits needed for successful employment in modern large-scale formal organizations of business and government. Restriction of their social experience almost exclusively to informal adolescent groups may well be a major factor in the failure of many delinquent youth to meet the requirements of jobs provided for them (Loeb, 1973; Glaser, 1978;178-81). This, of course, is in addition to the stigma of a criminal record and, in many cases, to disabling appetites.

## Drugs and Crime

Since ancient times various substances have been consumed by humans in order to alter their moods. Alcohol has probably always been the drug most widely used for this purpose, but opiates have a long history, as do marijuana and a derivative of the same plant, hashish. Cocaine and amphetamines also supplemented coffee and tea as stimulants in many times and places. Today 10% to 20% of the adult population in many communities take tranquilizers and sedatives under very liberal medical prescriptions or without this legitimation, although most of these commonly used substances are clearly more disabling than marijuana and some other legally prohibited mood-altering substances. Which one of these chemicals becomes legitimated and which outlawed has not been a function of the biological or psychological dangers inherent in their use, but of the political tactics of various interest and status groups (Goode, 1972; Glaser, 1978:Chapters 2 and 10).

The history of all of our drug prohibition legislation seems to be one of ineffectiveness, largely because the participants in drug use do not consider themselves victimized and therefore do not complain to the police. Consequently, only a small percentage of users and sellers are caught. When the substances are highly addictive, users have a very inelastic demand for them, making their sale so profitable that whenever law enforcement cuts off one source, others expand and new supply channels soon open. Also, when scarcities develop, some drugs are substituted for others. If the substances not only are highly addictive but their cost becomes more than most users can earn legitimately, they resort to crime to pay for drugs. Then police actions that make the supply short and the price high increase the amount of property crime and prostitution committed by addicts (Fuji, 1975: Votey and Phillips, 1976).

In addition, when prohibited substances are widely used, even by persons of influential social status, as alcohol was during our national Prohibition Era and marijuana increasingly has been in the past decade, enforcement of laws against them becomes glaringly haphazard. This development alienates enough of the population to nurture a gradually successful movement for regulation rather than prohibition of the substance, with responsibility for combatting injurious use transferred from the police to public health and education agencies. This has happened with alcohol, may be on its way with marijuana, and has been initiated with opiates through methadone maintenance programs. All such decriminalization reduced crime and other social costs from use of these substances, and probably diminished employment impairment as well. Unless this shift from prohibition to regulation accelerates, the high cost of youthful drug abuse will undoubtedly continue to be a major source of both drug and property crimes by youths, as well as an impediment of efforts to reduce their unemployment rates (these themes are elaborated and documented in Glaser, 1978:Chaper 10).

## CONCLUSIONS

To combat youth crime is largely futile unless an effort is also made to assure legitimate employment for youths. To deal effectively with both youth crime and youth unemployment in the United States today, however, major social, cultural and political developments must be taken into account. Perhaps the most relevant and least recognized of these developments are the separation of youth social worlds from those of adults, the differentiation of youths in school adjustment, and the futile criminalization of substance abuse.

The practical implication is that youth crime reduction in the United States today requires not only full employment, but imaginative experiments, recognizing that each innovation probably will be effective for only a segment of the total offense-prone population. The many possibilities include, for example: subsidizing part-time employment of youngsters in junior high and high schools conditional on their progress in part- or full-time schooling, placing such youth individually or in very small numbers at work in a large variety of adult workplaces, treating small groups of students of mixed age and achievement as collaborative teams competing not as individuals but for rewarded increase in the team's average scholastic and conduct attainments, involving youth in formal management committees of mixed-age composition for matters that affect their daily lives (e.g., managing their recreational facilities, planning their cafeteria menus), changing youth corrections establishments from large storage institutions to small places with well-mixed staff and inmates plus much rewarded hard work, making abuse of psychoactive drugs of all types primarily a public health system rather than a criminal justice concern, decentralizing police and courts to get them closer to their clientele and oriented more to involving than to fighting our younger generation. All such endeavors require analytic rather than the customary global evaluations; we need to learn what works for whom, under what circumstances and above all why or why not, rather than assessing each policy only on whether or not it is a panacea (since none is, and all are easily rendered ineffective or even counterproductive by poor personnel in key positions).

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# RACIAL DIFFERENTIALS IN MALE YOUTH UNEMPLOYMENT

By: Paul Osterman

## ABSTRACT

This paper seeks to understand the source of racial unemployment differentials for young men. The approach is to estimate a model of youth unemployment separately for black and white youth and to compare the results for the two groups. The model consists of an equation for the duration of a completed spell of unemployment and equations estimating the probability of quits and layoffs. The results indicate significant racial differences in unemployment duration which persist even after controlling for various personal characterictics and labor market demand. On the other hand, the probability of a layoff is not much higher for blacks than whites and blacks are found more prone to quit into unemployment.

The final section of the paper examines the consequences of unemployment and finds that for whites the experience of unemployment seems to have few long term effects. However, for blacks high unemployment has long-term adverse consequences and low unemployment seems to have long-term benefits.

## INTRODUCTION

The remarkably high unemployment rates experienced by black youth are probably the most serious blot on the recent economic record, and the recent reduction in overall unemployment has not appreciably eased the plight of young blacks.

To a certain extent the high unemployment rate experienced by black youth is due to causes common to all young people, the hiring practices of stable employers and the restlessness of youth, and will disappear with age.[1] The outstanding question is why the differential is so large between the experience of the two groups. This differential is the topic of this paper.

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[1] In 1976 the unemployment rate for Black men was 34% for 18-19 year olds, 20.7% for 20-24 year olds and 11% for 25-34 year olds. The comparable figures for whites were 15.5%, 10.9%, and 5.6%. Although these are cross-sectional data the pattern is also found in longitudinal samples and persists after controls for education and years in the labor force. In part this pattern results from a change in the behavior of youth--quits decline with age as do exits from the labor force--and in part from the reluctance of stable employers to hire teenagers. See Osterman (1978) and Lester (1954).

There are important mysteries associated with this differential. It is not enough simply to assert that black youth unemployment stands in a relatively fixed ratio to white unemployment and that therefore the high recent rates experienced by black youth are not unexpected. The fact is that the experience of black relative to white teenagers has been worsening. Particularly for males, in the mid-sixties a shift seems to have occurred and the relative position of blacks deteriorated.[2] Why this occurred is unclear. Some have suggested that labor supply shifts are the culprit. For example, a variable measuring the ratio of youth to adults in the labor force tends to be significant in time series regressions for black unemployment but not for whites. However, in the absence of a convincing story about demand it is not clear why a surplus of young blacks should increase their unemployment relative to whites instead of doing damage to both. Such a demand-side story would presumably emphasize discrimination, but here the mystery deepens. During the same period in which the unemployment situation of young blacks seems to have deteriorated, their earnings relative to whites improved. There is convincing evidence from a number of sources that rates of return in earnings functions have been roughly (though not entirely) equalized across races. Even in the absence of equalization the trend seems definitely positive. It is difficult to reconcile this "new labor market" with respect to earnings with the unemployment experience.

In addition to supply and demand explanations there are also some possible structural stories. One of these is the minimum wage, but I do not find it convincing. Even granted a disemployment effect, recent evidence does not suggest that the *ratio* of black to white unemployment has deteriorated because of minimum wage increases. Another possible explanation is suburbanization of employment. More so than adults, youth tend to work near home and if youth-intensive jobs have moved out of reach then even in the absence of discrimination black youth could be saturating the inner city job market. This seems possible, but it must be kept in mind that suburbanization is a trend which predates the mid-sixties and that youth-intensive jobs (service and trade employment) have not suburbanized as rapidly as manufacturing.

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[2] Between 1950 and 1959 the ratio of black to white unemployment for 18-19 year old males was 1.48. Between 1960 and 1965 the ratio was 1.75 and from 1966 to 1976 the ratio was 2.16.

This paper will address a considerably more limited set of questions than those posed above. Operating in the context of the "new labor market" findings and using the viewpoint and kind of tools generally associated with that analysis we will examine whether differences in background characteristics and tastes can explain the cross-sectional differences in the unemployment experiences of black and white youth. This investigation is important because if background characteristics can "explain away" the difference in black and white unemployment experiences to the same extent that they can earnings differentials, then this might imply less attention to the hiring practices of firms and more attention both to improving the endowments of black youth and to structural factors such as the suburbanization of employment opportunities. On the other hand, if it appears that black youth face different demand curves, then renewed efforts at equal employment opportunity programs and strengthening the placement capabilities of manpower programs and schools may seem important. To anticipate the answer, the results below show that after controlling for various factors the unemployment gap narrows but that an important fraction of this gap cannot be "explained away."

The next section of this paper will present and estimate a cross-sectional model of teenage unemployment, using microdata

Following that, in the final section, I will briefly discuss a topic to which surprisingly little attention has been given, the long-term consequences of teenage unemployment and how this varies by race.

## A MODEL OF YOUTH UNEMPLOYMENT

The idea underlying this approach is to estimate a model of youth unemployment separately for blacks and whites and then to compare the structure of that model across the two groups. In this manner we can try to determine to what extent the different outcomes are due to different values of the parameters, i.e., differences in treatment of behavior, and how much they are due to differences in the values of the variables, i.e., differences in endowments.

Modern theories of unemployment distinguish between spells and duration. A group may suffer higher unemployment rates either because it has a disproportionate number of spells, though each spell may end quickly, or because unemployment lasts a very long time when it does occur. Important differences in interpretation arise, with long durations implying difficulty finding work while frequent spells are thought to imply either unstable behavior or high risk jobs. I will first analyze the determinants of duration and then turn to an analysis of spells.

### Duration of Unemployment

The model employed here is drawn from the implications of various search theories (for example, Mortensen, 1970 and Lippman and McCall, 1976). An individual's duration of unemployment is determined by a two equation system, one equation determining the reservation or acceptance wage, the other determining the duration of a spell. This system can be summarized as:

(1) $R = R\ (K,D,C)$

(2) $D = D\ (U,R,K)$ D = Duration of spell

K = Skill Level

R = Reservation Wage

C = Cost of search and time spent unemployed

U = Distribution of job vacancies

with:

$\frac{\partial R}{\partial K} > 0$ $\frac{\partial R}{\partial D} < 0$

$\frac{\partial R}{\partial C} < 0$ $\frac{\partial D}{\partial U} > 0$

$\frac{\partial D}{\partial R} > 0$ $\frac{\partial D}{\partial K} < 0$

A straightforward approach to searching for racial differentials in duration is to estimate this system for blacks and whites and test for the differences across the equations. In particular, it would be interesting to know whether there are differences in the rate at which the reservation wage falls in response to duration, differences in the extent to which skill levels reduce duration, and differences in the impact of cost reducing factors, such as unemployment insurance, in increasing the reservation wage.

Estimates of this system will be provided below. However, there is a serious difficulty. Although this system provides a measure of the determinants of reservation wage, the measure of duration is seriously biased. Rather than being a measure of completed spells, which is what we want to measure with respect to our complete system of unemployment, the simultaneous system above provides a measure of spells in progress. As has been shown (Marston, 1975), use of duration in progress provides a biased estimate of the length of completed spells largely because long spells have a higher probability than short spells of being sampled at a point in time.

Because of this problem the emphasis in this paper will be on a reduced form of (1) and (2), namely

(3) $D = D(U,K,C)$

where D is now length of completed spells. Unfortunately, this introduces some ambiguity into both the expected signs of the variables and the interpretation of the coefficients. For example, a high skill level would reduce duration by making more vacancies accessible but might also increase duration by raising the reservation wage. Furthermore, a positive coefficient on marriage, for example, may be due either to married workers being more eager to find work (and thus reducing their asking wage) or to them being more attractive to employers and hence receiving more or better offers.

The data employed are those in the National Longitudinal Survey of Young Men (Parnes, 1970).[3] Information from two survey periods are employed, 1969-1970 (collected in 1970) and 1970-1971 (collected in 1971). These periods were chosen because they are the first for which complete information is available on every job held and on each spell of unemployment. The sample was limited to whites and blacks (other nonwhite being excluded). The analysis is limited to out-of-school youth.[4]

In the analysis of the reduced form equation (3) the unit of observation is each completed spell of unemployment which occurred between 1969-1971. This procedure insures that we are measuring the

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3. The NLS data, for reasons not yet understood, report unemployment rates below those reported by the monthly Census and hence may seem a poor data source for examining unemployment. However, the racial ratios are very similar to those in the Census. For example, the October 1970 Current Population Survey reported racial ratios for out-of-school male 18-19 year olds of 1.84 and for 22-25 year olds 1.65. The NLS ratios for the same period were 1.88 and 1.66 respectively.

4. No observation of a spell is included if it occurred while the youth was in school. However, because observations are pooled, as will be explained below, some youth in the sample were in school during some portion of the period.

theoretically proper dependent variable.[5] In addition, only spells associated with job changing, entrance, or re-entrance into the labor market are included. Thus spells associated with temporary recalls are excluded. Whatever the importance of this class of spells for adults (Feldstein, 1975) they are not important for youth[6] and they are excluded because of expected differences in the pattern of job search.

The independent variables are defined as follows:[7]

Skills and Personal Characteristics

AGE: Age in years, measured at the beginning of the year.

KWW: This is the score on a knowledge of the world of wor test administered by the interviewers. In addition to the possible direct importance of such a measure in explaining ability to find a job, it is also a good proxy measure of intelligence (Griliches, 1976).

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[5] Every spell which occurred any time between the 1969 and 1971 interviews is included. The only exceptions are spells in progress at the time of the 1971 interview. Those are excluded because information on their length is unavailable. Thus the measure employed here seems to be the closest possible approximation of the theoretically appropriate variable. There is still some bias since a very long spell, say one which began at the time of the 1969 interview and was still in progress at the time of the 1971 interview, would be excluded. However, the fact that the sample period is over two years long makes this bias of little practical importance since there is plenty of opportunity to capture long spells. For further discussion of this issue see Welch (1977).

[6] In the entire sample there were only 68 affirmative responses in the 1969-1970 period to the question "Did you experience a spell of unemployment while holding this job?"

[7] In these definitions the term "year" should be understood to refer to the interview period, either 1969-1970 or 1970-1971. When a variable is described as measured at the beginning of the year this means at the time of the 1969 interview if the spell occurred during 1969-1970 and at the time of the 1970 interview if the spell was in the 1970-1971 period.

EDUCATION: Years of education, measured at the beginning of the year.

DEPEN: Number of dependents, excluding the wife, measured at the beginning of the year.

DRAFT: "1" if eligible for the draft, "0" if not, measured at the beginning of the year.

MAR: "1" if married at the beginning of the year, "0" if not.

### Search Cost Variables

UI: This is the fraction of wages replaced by unemployment insurance. The variable is (Total UI dollars received) /(Hourly wage of most recent job x 35 x weeks unemployed that year) and it is defined analogously to that employed by Ehrenberg and Oaxaca (1976). It is measured with error since data on the amount received is available for the entire year but not for each spell. Also, see footnote 9.

NONWG: This is nonlabor income (excluding transfer payments) received during the year. The availability of such income should permit, and perhaps encourage, more extended search.

### Demand Variables

U: The local unemployment rate, measured in tenths of a point.

DU: The local unemployment rate at the end of the year minus the rate at the beginning of the year.

Other Variables

LINE: "1" if the spell began when the respondent left a previous job and if upon leaving the respondent had the next job lined up in advance, "0" otherwise.

LAYOFF: "1" if the spell began with a layoff from a previous job, "0" otherwise.

OLF: "1" if during the spell of unemployment time was also spent out-of-the labor force, in addition to time unemployed. "0" if not. Time out-of-the labor force spent in school or in the armed forces is not included in this measure.

An important peculiarity of the youth labor market is that an important fraction of unemployment is associated with entering and re-entering the labor force. This pattern is largely due to the work rhythm imposed by the school calendar and the process of leaving school. Spells of unemployment due to entrance and re-entrance are likely to differ from spells caused by job leaving or loss and hence are analyzed separately. The discussion here will emphasize non-entrance or re-entrance spells.[8]

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[8] Twenty percent of the total spells in the sample were due to entrance or re-entrance. A Chow test was performed to test the hypothesis that entrants/re-entrants and job leavers share the same equation and the hypothesis was rejected at better than the .01 level. Within the entrance/re-entrance group there were no significant racial differences and a Chow test failed to reject the equality hypothesis. The results for the analysis for entrants and re-entrants are available upon request.

The results of the duration equation are presented in Table 1. The two racial equations were tested for equality via the Chow test and the hypothesis of equality was rejected at the .05 level (F = 2.3). In addition, race was fully interacted with the variables in a pooled equation to test for significant differences among specific coefficients. The coefficients of DRAFT, U, and LINE are significantly different at the .05 level and the coefficients of AGE differ at the .10 level.

The interpretation of these coefficients must be tempered by the realization that the equation is a reduced form. Keeping this limitation in mind, there are several interesting results. Time spent out of the labor force, neither working nor looking, reduces the duration of unemployment. This is plausible since many jobs are found through word of mouth, and the word can easily be passed to someone not actively looking. Thus for youth the distinction between time unemployed and time out-of-the labor force can be tenuous. The impact of unemployment insurance seems marginal, and coefficients in both equations are insignificant and of opposite sign. However, as noted above, this variable is measured with potentially serious error and there are additional possible biases in its use.[9] A change in the unemployment rate (DU) has an identical impact on black and white duration, although the level of unemployment has

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[9] A spurious positive correlation between duration and benefits is caused by the fact that the NLS data do not tell us whether a worker is covered, only that he received benefits. Most states have waiting periods, and thus a minimum spell length is required for even a covered worker to receive benefits. See Welch (1977).

## TABLE 1

## DURATION EQUATIONS
(Standard Errors)

| Variable | Black | White | Variable | Black | White |
|---|---|---|---|---|---|
| AGE | 0.511<br>(0.206) | 0.006<br>(.174) | DU | 0.055<br>(0.031) | 0.055<br>(0.029) |
| KWW | -0.024<br>(0.079) | -0.013<br>(0.059) | UI | -0.271<br>(0.466) | 0.268<br>(0.585) |
| DRAFT | 5.141<br>(1.474) | 0.055<br>(0.029) | NONWG | 0.001<br>(0.007) | 0.003<br>(0.001) |
| DEPEN | 0.402<br>(0.449) | -0.092<br>(0.601) | OLF | -3.303<br>(1.589) | -2.385<br>(1.059) |
| EDUCATION | 0.535<br>(0.255) | 0.223<br>(0.210) | CONSTANT | -10.140 | 1.600 |
| MAR | -3.246<br>(1.393) | -1.577<br>(1.049) | N | 247 | 412 |
| | | | F | 3.421(13,233) | 2.44(13,398) |
| LAYOFF | 3.243<br>(1.106) | 0.643<br>(0.841) | SE | 8.395 | 7.952 |
| | | | $\bar{R}^2$ | .113 | .043 |
| LINE | 2.314<br>(1.893) | -2.598<br>(1.353) | | | |
| U | -0.003<br>(0.028) | 0.073<br>(0.022) | | | |

opposite racial effects.[10] Finally, non-labor income increases the duration of spells for both races, although the effect is statistically significant only for whites.

A useful technique for summarizing the results of these equations is to decompose the differential into portions due to differences in the values of the variables of the two groups and differences due to the structure of the equations. This decomposition is reported in Table 2.[11] In this decomposition negative items are favorable to whites. The results indicate that difference in the structure of the equations implies that blacks have durations which are 1.200 weeks longer than they would be were they treated or behaved as whites. Given that their actual duration is 7.915 weeks they suffer durations which are 17% "too long". On the other hand, black characteristics are slightly "favorable". In particular, they have less education and non-wage income than do whites, both of which increases white duration. The sum of these favorable characteristics reduces their durations relative to whites by .403 weeks and this, when subtracted from the differential due to equation differences, leads to an actual differential of .797 weeks.

Because this estimate grows out of a reduced form (Equation 3) it is unclear how much of the unexplained racial gap is due to be-

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[10] In cross-sectional data the level variable may capture long run-equilibrium behavior. This is because of the very high correlation over time of an area's unemployment rate. Thus in this equation the level unemployment rate may serve as a proxy for structural characteristics of the local economy. Since high unemployment areas also tend to have high hourly wages the welfare interpretation is ambiguous.

[11] The formula is:

$$D = (\beta_0^W - \beta_0^B) + \Sigma\beta_0^W(\overline{X}_i^W - \overline{X}_i^B) + \Sigma\overline{X}_i^B(\beta_i^W - \beta_i^B)$$

where D is the total differential, i indexes the variables, and the superscripts refer to blacks and whites. The first and third terms on the right represent differences in the structure of the equations while the middle term represents differences in the values of the variables.

## TABLE 2

## DECOMPOSITION OF UNEMPLOYMENT DURATION

| Variable | Difference Due to Characteristics | Difference Due to Equation Structure |
|---|---|---|
| AGE | -.003 | -10.988 |
| KWW | -.086 | .297 |
| U | .018 | 3.559 |
| DU | .116 | 0 |
| DEPEN | .057 | -.513 |
| EDUCATION | .258 | -3.294 |
| UI | .014 | .079 |
| DRAFT | -.002 | -1.087 |
| LAYOFF | .048 | -1.152 |
| NONWG | .124 | .021 |
| LINE | -.015 | - .455 |
| MAR | -.012 | .581 |
| OLF | -.114 | .012 |
|  | .403 | -12.940 |
| CONSTANT |  | 11.740 |
|  |  | -1.200 |

havioral differences and how much difference in treatment. The most important likely behavioral difference is the relationship between duration and reservation wage depicted in Equation 1 above. If young blacks have an "unreasonably" high reservation wage [12] or if their reservation wage declines less rapidly than it does for whites in the face of unemployment [13] then the consequence would be longer duration.

Table 3 presents a two-stage least squares estimate of the reservation wage equation for youth unemployed at the time of the 1970 or 1971 surveys. [14] All of the variables have been defined previously, with the exception of EXXP which is (AGE-EDUCATION-5). This is the standard experience variable employed in many earnings functions. The sample is limited to youth out-of-school at the time of the unemployment who were unemployed for reasons other than temporary lay-off, waiting for a new job to begin, or a labor dispute.

The results in Table 3 imply that little of the observed difference in duration can be attributed to differences in reservation wage formation. The coefficient on duration is only 5¢ per week apart for

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12. A too high reservation wage might occur because blacks, perhaps due to inadequate information, over-value their potential earnings. Another possibility is that blacks, perhaps due to changing attitudes, are refusing to take jobs which offer them wages below that which comparable whites would earn. This shift in the supply curve could also explain the equalization of earnings found in earnings equations since these are actually reduced forms of supply and demand equations.

13. In fact most studies of youth find they take the first job offered (Stephenson, 1976). However, the reservation wage mechanism might operate through patterns of search. If youth have information about the characteristics of firms they may search only among firms whose entry wage is equal to or better than their reservation wage.

14. The duration equation is available upon request.

## TABLE 3

## TWO STAGE LEAST SQUARES RESERVATION WAGE EQUATIONS
(Standard Errors)

| | WHITES | BLACKS |
|---|---|---|
| DURATION | 2.471<br>(6.411) | 7.025<br>(4.384) |
| MAR | 69.296<br>(28.99) | -26.195<br>(36.011) |
| LINE | -100.162<br>(154.009) | 3.994<br>(82.679) |
| DRAFT | 41.385<br>(24.989) | -29.418<br>(27.549) |
| OLF | -37.601<br>(21.958) | 55.067<br>(27.565) |
| EXXP | 22.149<br>(16.245) | 6.451<br>(10.750) |
| EXXP2 | -.890<br>(1.495) | -.163<br>(.805) |
| UI | -9.601<br>(8.735) | 32.963<br>(21.073) |
| KWW | .215<br>(1.576) | 3.538<br>(2.228) |
| DEPEN | 33.625<br>(20.259) | 10.155<br>(13.984) |
| EDUCATION | 21.507<br>(5.121) | -2.626<br>(7.845) |
| CONSTANT | -95.572<br>(70.105) | 64.831<br>(105.36) |
| $R^2$ | .511 | .316 |
| F | 5.71(11,60) | 1.88(11,45) |
| S.E. | 74.7739 | 73.846 |
| N | 72 | 57 |

Note: The dependent variable is the hourly reservation wage in cents.

the two races.[15] When black mean values were substituted into the white equation the predicted reservation wage is $2.41 an hour, slightly higher than the actual value of $2.38. Thus the white and black reservation wage structures are essentially the same and there is no evidence that black youth unemployment is due to unrealistically high reservation wages.

Thus the key conclusion of this section, that an unexplained racial gap which implies durations of 17% "too long" for blacks, seems not to be due to behavioral differences, at least those captured in reservation wage behavior.

## Spells of Unemployment

In this section we will examine spells of unemployment and seek to disentangle the effect of differences in endowments on the one hand and differences in treatment and behavior on the other. The most important conclusion which emerges from this effort is that, unlike the case of duration, the racial differences in spells of unemployment are largely due to either differences in background characteristics or behavior, and apparently not to differences in treatment.

The natural approach to this question is to estimate separate racial equations for the probability of a spell of unemployment and to compare the equations. However, it is important to distinguish between quits and layoffs. The same variable, for example the unemployment rate, has an opposite expected impact upon quits and layoffs and, therefore, we will estimate a separate model for each.

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[15] The positive, though insignificant, sign on the coefficients is contrary to that predicted by theory. Evidently for this sample duration has no effect on reservation. When the system was estimated using time not working (i.e., duration of unemployment plus time out-of-the labor force) the coefficient was 2.8, with a standard error of 3.7 for blacks and -4.6 with a standard error of 7.8 for whites.

Not all quits and layoffs lead to unemployment. In both instances, though presumably more so for quits, a separation can be followed by immediate acquisition of another job. Furthermore, the separation can also be followed by movement out-of-the labor force. The distribution of quits and layoffs into these categories is shown in Table 4. The figures in the table are the average of the 1969-1970 and 1970-1971 quit and layoff rates for youth out of school in those periods and the data reflects all job changes which occurred in those periods.

As is apparent from lines 4 and 8, both the overall layoff and the quit rates are higher for blacks than whites, the quit rate being 7% higher and layoff rate 20% higher. These results are not surprising; we expect to find blacks laid off more frequently than whites both because of discrimination and because of lower endowments. Furthermore, the poorer jobs held by blacks would lead them to quit more frequently. What is surprising about this table is that if one restricts attention to layoffs and quits resulting in unemployment, then while the differential remains roughly the same for layoffs (23%) it widens considerably for quits to 62% higher than the white rate. Evidently while blacks do not quit in general much more frequently than do whites, they are considerably more prone to quit into unemployment and less prone to quit and immediately find another job. It remains, of course, to see if this pattern persists after controlling for differences in personal characteristics.

Parenthetically, it is also interesting to note that this table supports the common view that most voluntary job changing does not result in unemployment. However, it is surprising to learn that a considerable fraction of layoffs (46.8% for whites and 44.2% for blacks) are also immediately followed by another job.

Our next step is to estimate quit and layoff models. In the analysis which follows attention will be limited to only those quits

## TABLE 4

## ANNUAL QUIT AND LAYOFF RATES

| | Whites | | Blacks | |
|---|---|---|---|---|
| | Rate | Percent | Rate | Percent |
| Layoffs | | | | |
| (1) Resulting in Unemployment | .073 | 46.2 | .096 | 50.5 |
| (2) Resulting in Labor Force Withdrawal | .011 | 6.9 | .010 | 5.2 |
| (3) Followed by Another Job | .074 | 46.8 | .084 | 44.2 |
| (4) | .158 | 100 | .190 | 100 |
| Quits | | | | |
| (5) Resulting in Unemployment | .072 | 22.1 | .117 | 34.0 |
| (6) Resulting in Labor Force Withdrawal | .035 | 10.7 | .033 | 9.5 |
| (7) Followed by Another Job | .218 | 67.0 | .194 | 56.3 |
| (8) | .325 | 100 | .344 | 100 |

Note: The rates are averaged for two periods, 1969-1970 and 1970-1971. Only out-of-school youth are included. Percentages may not add up to 100 due to rounding.

and layoffs followed by spells of unemployment. The quit equations can be motivated by search theory, human capital theory, or some amalgam of the two. Accumulation of specific human capital and high wages should, holding the other variables constant, reduce the probability of quitting (Parsons, 1972). Potential other opportunities, indexed in this model by the unemployment rate, should increase the probability of quitting. Personal characteristics, such as marital status, dependents, and age, have an ambiguous effect depending upon their impact on the individuals' needs and taste for risk.

The variables in the quit equation which are new are TENURE, which measures years on the job, UNION, a dummy variable which takes on the value of "1" if wages are set by collective bargaining and "0" otherwise, and WAGE, the hourly wage measured in cents. In addition, a variable is introduced to test the hypothesis that one source of black quitting is discrimination on the job. The variable WTDIF is constructed by fitting a wage equation for whites, estimating what each individual would receive, and taking the difference between that value and the actual hourly wage.[16] A positive value for Blacks would indicate a wage below that predicted by the white equation and may be correlated with quitting.

In the layoff model the expectation is that specific human capital and a high skill level will reduce the probability of a layoff while increases in the unemployment rate should increase the probability (Oi, 1962). Temporary layoffs are again excluded from the analysis.

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[16] The auxiliary equation was:

$$\ln(\text{hourly wage}) = B_0 + B\,\text{EDUCATION} + B_2\text{KWW} + B_3\text{TENURE} + B_4\text{UNION} + B_5\text{TENURE}^2 + B_6\text{EXXP} + B_7\text{EXXP}^2 + B_8\text{MAR}$$

See footnote 21 for a brief description of Flanagan's (1978) use of a similar variable.

In all the equations the dependent variable is dichotomous and takes on the value of "1" if a quit or layoff followed by unemployment occurred during the year and "0" otherwise.[17] As was the case in the duration analysis, the sample is pooled for two years, 1969-1970 and 1970-1971, and only youth out of school at the beginning and end of the period are included.

The equation was estimated to fit the logit functional form[18]

$$P = 1/1+e^{-BX}$$

where P is the dichotomous dependent variable, X is the vector of explanatory variable and B are the estimated parameters. A maximum likelihood estimation procedure was employed. The results of the logit quit and the layoff equations are presented in Table 5. Predicted probabilities for the mean values of the variables are provided in Table 6. The story told by this equation is somewhat surprising.[19] The uncontrolled gap for layoffs (Table 4) narrows but the gap remains. Thus even after controlling for personal characteristics, demand, experience, and job skills young black men still face a higher layoff probability, but the gap is not strikingly large. With respect to quits we find the surprising result that the gap widens. Blacks seem considerably more prone to quit into unemployment.

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17. The incidence of multiple quits or layoffs during one year is very low. During 1969-1970 only 1.2% of whites and .7% of blacks experienced more than one layoff and for quits the figures are 1.1% and 1.4%.

18. The logit functional form is convenient because it constrains predicted values to a (0,1) interval, a characteristic which is essential if the equation is to have a probability interpretation. In addition, McFadden (1973) has shown that under reasonable assumptions the logit form follows from utility maximizing behavior.

19. Ordinary least squares estimates of linear probability models produce equivalent results. For example, for the quit equation the black values substituted into the white equation produce a predicted quit rate of .049.

## TABLE 5

## QUIT AND LAYOFF EQUATIONS

(absolute value of t statistics in parentheses)

| | Quit | | Layoff | |
|---|---|---|---|---|
| | Black | White | Black | White |
| CONSTANT | -2.178<br>(2.074) | 0.208<br>(0.185) | -1.740<br>(1.918) | -0.461<br>(0.584) |
| AGE | 0.055<br>(1.033) | -0.082<br>(1.499) | -0.026<br>(0.679) | -0.003<br>(0.089) |
| EDUCATION | 0.153<br>(1.735) | -0.148<br>(1.741) | -0.033<br>(0.579) | -0.198<br>(4.418) |
| TENURE | -0.037<br>(0.584) | -0.619<br>(4.736) | -0.243<br>(2.881) | -0.514<br>(6.491) |
| DRAFT | 0.556<br>(1.854) | -0.338<br>(1.108) | 0.255<br>(0.839) | 0.087<br>(0.323) |
| DEPENDENTS | 0.025<br>(0.265) | -0.157<br>(1.262) | - | - |
| MAR | 0.368<br>(1.044) | -0.324<br>(0.923) | -0.428<br>(1.818) | -0.449<br>(2.252) |
| UNION | 0.293<br>(0.527) | 0.045<br>(0.091) | -0.714<br>(2.483) | 0.428<br>(2.215) |
| WAGE | -0.016<br>(2.365) | -0.001<br>(0.248) | 0.002<br>(2.504) | 0.0001<br>(0.135) |
| KWW | 0.034<br>(1.494) | 0.040<br>(1.937) | 0.0006<br>(0.036) | -0.003<br>(0.297) |
| U | -0.001<br>(0.248) | 0.010<br>(1.943) | 0.002<br>(0.325) | 0.022<br>(4.986) |
| DU | 0.015<br>(1.787) | 0.010<br>(1.811) | 0.038<br>(4.061) | 0.012<br>(2.548) |
| WTDIF | 0.014<br>(2.143) | 0.0006<br>(0.100) | - | - |
| - 2 *log likelihood | 554.318 | 807.896 | 550.046 | 989.624 |

## TABLE 6

## PREDICTED PROBABILITIES OF QUITS AND LAYOFFS FOR BLACKS

| | White Equation | Black Equation |
|---|---|---|
| Quits | .034 | .120 |
| Layoffs | .078 | .089 |

In terms of the mechanics of the equations the key variables are AGE, EDUCATION, TENURE, AND WAGE. Increases in the values of the first three variables all decrease the probability of white quits but increase the probability of black quits. The only offset is WAGE in which a high wage decreases the probability of black quitting more so than is the case for whites.

The variable employed to test for quitting due to differential treatment, WTDIF, performs as expected both in its sign and significance. This is interesting because it lends support to the notion of shifts in supply as well as demand curves (footnote 12); however, the magnitude of the effect is small.

The explanation for the quit differential is elusive. The differential in overall quits, as opposed to quits into unemployment, is considerably smaller and hence (as we will see below) the structure of a general quit equation may be more similar across the races than the unemployment quit equation. Thus blacks may quit not much more frequently than whites but they have difficulty lining up the next job. Possible reasons for this will be mentioned in the conclusion, but this is a good point to summarize the results of the entire system of equations.

Table 7 summarizes the results of the several duration and spell equations and indicates how much additional unemployment can be attributed to each of the divergences between the actual black values and the value predicted by each of the white equations. As is apparent in each instance-- quits, layoffs and duration-- blacks experience more unemployment than they would had they been treated (or behaved) like whites. The difference in quit behavior accounts for nearly half of the additional unemployment, followed in importance by layoffs and duration. In the period 1969-1971 the average annual weeks of unemployment for out-of-school youth was 4.145 for Blacks and 2.252 for whites, thus the differential was 1.893 weeks. Column 4 of Table 7 shows the fraction of the total differential

## TABLE 7

## SUMMARY OF RACIAL DIFFERENTIALS FOR THE FULL SYSTEM

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| QUITS | .096 | .034 | .490 | 25.8% |
| LAYOFFS | .117 | .078 | .308 | 16.2% |
| DURATION | 7.915 | 6.715 | .255 | 13.4% |
| | | | | 55.4% |

Note:

(1): Actual Values for Blacks.

(2): Predicted Values Using White Equations and Black Characteristics.

(3): Extra Annual Weeks of Unemployment Due to Divergence of (1) and (2), Holding Remaining Variables Constant.

(4): Fraction of Annual Differential in Annual Weeks of Unemployment Accounted for by (3).

accounted for by the difference between actual values and those predicted by the white equations. This calculation implies that 55% of the total differential is due to differences in behavior or treatment while the remaining 45% can be explained by differences in personal characteristics.

As a final point, the reader may wonder why so little has been made of the role of distribution among industries. It is often thought that one reason blacks experience high unemployment is that they are concentrated in unstable industries. This, however, is not an important issue in these data. For example, in 1969-1970 blacks out of school at both the beginning and end of the period averaged 3.39 weeks of unemployment. Had they had the same industrial distribution as whites (but with the black within industry unemployment experience) they would have averaged 3.32 weeks of unemployment. The difference is small. Furthermore, the inclusion of industry dummy variables in the quit and layoff equations had no appreciable effect on the results.

## DISCUSSION AND FUTURE TOPICS

The results presented here have shown that an important fraction of the racial differential remains after the various controls, that the most important source of this unexplained differential lies in quit behavior but that important components are also due to layoffs and duration.

The most puzzling finding is clearly the quit differential: layoffs and duration differences can plausibly be attributed to discrimination in firing and hiring. We will offer some additional explanations below, but first it is worthwhile to explore the quit issue a bit further (although we will not be able to resolve it).

It does not seem to be the case that blacks quit in general more frequently than do whites. We saw in Table 4 that the over-all quit rate of young black men is not appreciably higher than that of whites. We estimated via ordinary least squares a linear probability model for all quits (employing the same variables found in Table 5) and when black mean values were substituted in the white equation the predicted probability of a quit was .280 higher than the actual black mean of .264.[20] This stands in contrast with both the logit and the OLS results for quitting into unemployment.

Thus young black men are not especially quit prone. Rather it is that their quits are more likely to lead to unemployment than those of whites. One possible explanation might be found in the motives for quitting, but Table 8 seems to dispel this possibility. There is a clear difference in the reasons for quitting for those quits followed and not followed by unemployment. For example, the category "found a better job" is more important (for both races) for quits not followed by unemployment than by those which are. However, there do not seem to be major racial differences. For example, the nature of the job was cited by 51.9% of the blacks and 50.79% of the whites who quit into unemployment. Blacks do cite wages more often while whites cite working conditions but the impact

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[20] This mean is below that in Table 4 because that Table includes multiple quits by one individual while the dependent variable in the regressions was only dichotomous. The equations reported here are available upon request.

### TABLE 8

### REASONS FOR QUITTING

| | Blacks | Whites |
|---|---|---|
| Followed by Unemployment | | |
| Nature of Job | | |
| Hours/kind of work/conditions/ interpersonal/location | 36.8% | 44.6% |
| Wages | 15.1% | 6.1% |
| Found Better Job | 10.4% | 9.1% |
| Health | 8.5% | 14.4% |
| Other* | 29.2% | 25.7% |
| Not Followed by Unemployment | | |
| Nature of Job | | |
| Hours/Kind of work/conditions/ interpersonal/location | 22.3% | 20.5% |
| Wages | 17.6% | 11.6% |
| Found Better Job | 26.8% | 24.0% |
| Health | 6.0% | 4.4% |
| Other* | 26.7% | 38.9% |

* Other includes return to school, military, prison, family and personal, and other.

of this is unknown.[21]

Another possible explanation of the differential may lie in different access to job contacts. Having decided to leave a job whites may simply be more able than blacks to line up the next job without experiencing unemployment. This may also be an explanation of the difference in the length of durations. Most jobs are found through personal contacts (Granovetter, 1976) and if blacks have fewer personal contacts than whites they may experience difficulty locating jobs. Statistical controls for personal characteristics may fail to capture this important "unobservable" and since personal contact networks help people of every education and skill level land jobs (Granovetter, 1976) it is unlikely that the effect of this variable would be fully captured by other measured variables. The consequence may be that blacks, even after controlling for personal characteristics, may still experience a harder time than whites in finding a job.

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[21] In a useful paper published after this article was completed Flanagan (1978) examined racial differences in quit and layoff experiences. The focus of the article was upon a variable similar to the WTDIF variable employed here and his results were generally comparable. As is the case here Flanagan found that the probability of quitting into unemployment is higher for young blacks than whites. He argued that this is due to an inverse relationship between time intensive search methods and wage rates (and skill levels). Low wage rates make time intensive search more desirable than goods intensive search. Because young blacks have lower average wage levels Flanagan argued that they more often choose to be unemployed while searching (i.e. use their time to search). However, this argument is not satisfactory. First, the NLS data do not reveal significant racial differences in job finding and job search methods (see below). Secondly, the quit equations reported here show a different structure across the races while Flanagan's arguement would imply a similar structure with the observed differences emerging from the different values of the variables (wage level and personal characteristics) which would lead to different choices of search behavior.

In the NLS data blacks and whites reported essentially the same pattern of job finding. For example, for youth out of school in 1969, 51.3% of the blacks and 44.2% of the whites found their jobs through personal contacts.[22] However, assuming that the peers and family of youth are similar to themselves, a smaller fraction of these contacts for blacks will be employed than for whites and thus the network may be less efficacious. Some evidence of this is found in interviews we conducted in Boston. The sample consisted of 150 non-college out of school males between the ages of sixteen and twenty-six, half from a white working class community (East Boston) and half from a black community (Roxbury). Of those who reported finding their current job via personal contacts 70% of the whites said that the contact was working in the firm where the job was found while for blacks the figure was only 42%. Thus contacts for whites may simply be more effective.

This argument clearly is tentative and requires more research, as does the general issue of the quit differential between blacks and whites. Thus, though we have come some way toward understanding the nature of the racial differential in unemployment, there is clearly more to learn. It should also be recognized that the results here are cross-sectional and thus cannot well explain changes in the differential over time. These shifts are important since in the mid - 1960s the ratio of black to white youth unemployment took a sudden adverse shift for the worse, a shift to a level which has remained with us. A satisfactory understanding of this would require, at least in part, that we shift our attention away from the personal characteristics and behavior emphasized in this article and focus upon structural changes in the economy; changes such as the suburbanization of jobs, rising labor force participation of adult women, and changes in the structure of demand for youth labor.

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[22] Direct contact with the employed was used by 24.9% of the blacks and 22.8% of the whites. Newspaper ads were employed by 4.2% of the blacks and 5.7% of the whites.

## THE CONSEQUENCES OF UNEMPLOYMENT

We know surprisingly little about the long-term consequences of teenage unemployment. Are teenagers who experience considerable unemployment handicapped later in their careers or is the effect transitory and unimportant? Arguments that the impact is considerable down the road could rely either on the assumed psychological impact of joblessness early in the working career or upon the missed opportunities to develop training and skills. On the other hand, the association of unemployment with moratorium behavior and the importance of new and re-entrants in unemployment would tend to imply that there are few long-term effects and that the real problem is one of lost output and income maintainence in the short run.

Little work has been done on this question and hence little can be said with confidence. An initial effort at shedding some light on the issue is presented in Table 9.

In this table the young men in 1968 are divided into three groups: those who had no unemployment in 1968, those who experienced between one and four weeks, and those who experienced five or more weeks. Within each of these three groups we look at their unemployment experience in 1966 and 1970 and determine the deviation of their unemployment in those years from the average unemployment experienced by the entire group (or subgroup) in that year. Thus, for example, whites with no unemployment in 1968 had 26% less unemployment in 1966 than all whites experienced in 1966 and 22% less unemployment in 1970 than all whites. Or, to pick another example, blacks with less than a high school degree who experienced one to four weeks of unemployment in 1968 had 12% more unemployment in 1966 than all blacks with less than a high school degree experienced that year and in 1970 had 91% more unemployment than all blacks with less than a high school degree had that year.

The advantage of looking at 1966 as well as 1970 unemployment is that the 1966 experience was obviously unaffected by the 1968 experience

## TABLE 9

## DEVIATIONS FROM EXPECTED UNEMPLOYMENT IN 1966 AND 1970 BY WEEKS UNEMPLOYED IN 1968

| | 0 Weeks, 1968 | | 1-4 Weeks, 1968 | | 5 or More Weeks, 1968 | |
|---|---|---|---|---|---|---|
| | 1966 | 1970 | 1966 | 1970 | 1966 | 1970 |
| Whites | -26% | -22% | +77% | +35% | +266% | +126% |
| Blacks | -12% | -34% | +25% | +38% | + 36% | + 81% |
| 16-19 Years | | | | | | |
| Whites | -47% | -48% | | | | |
| Blacks | - 6% | -43% | | | | |
| 20-23 Years | | | | | | |
| Whites | - 7% | + 6% | +37% | - 2% | | |
| Blacks | 0% | -45% | -11% | +37% | | |
| 24-26 Years | | | | | | |
| Whites | -40% | -25% | +74% | +123% | | |
| Blacks | -23% | -10% | | | | |
| < High School Degree | | | | | | |
| Whites | -29 | -16% | +44% | -17% | +219% | |
| Blacks | - 9% | -42% | +12% | +91% | + 16% | + 53% |
| High School Degree | | | | | | |
| Whites | -17% | -33% | +62% | +107% | | |
| Blacks | -17% | -25% | | | | |
| More than HS | | | | | | |
| Whites | -51% | -11% | | | | |
| Blacks | 0% | -21% | | | | |

Age is as of 1968; Education as of 1970; Only those out-of school in 1967-68 included; Data not reported for cells less than 30.

and hence we can control for the unobserved differences in individuals which might have led them to experience more or less than average unemployment in both 1968 and 1970. Thus, to determine the impact of 1968 unemployment we look at the difference between the 1966 and 1970 figure.

The lesson from this table is quite striking. Considerable unemployment in 1968 does not adversely affect whites in 1970 but it does damage blacks and, conversely, little unemployment in 1968 does not help whites in 1970 but it does help blacks. This result holds for virtually all age and education groups.

This point can be made clearer if we look, for example, at the rows labled "whites" and "blacks." Whites who had no unemployment in 1968 had 26% less than average in 1966 and 22% less than average in 1970. Thus this group is clearly more successful than the average for whites but the zero unemployment in 1968 did not appreciably improve the situation in 1970. On the other hand, for blacks, those with zero unemployment in 1968 had 12% less than the average in 1966 and 34% less than the average in 1970. Thus, while the 1966 figure indicates that the group is somewhat special, the 1970 figure is an improvement beyond expectations.

A similar picture emerges for groups with high 1968 unemployment. Thus, among all whites with five or more weeks in 1968 the 1966 figure indicates that the group is in difficulty to begin with but the 1970 figure is an improvement and is a regression to the mean. On the other hand, for blacks with five or more weeks in 1968 the 1966 figure also indicates trouble independent of the 1968 experience, but the 1970 figure is a worsening of the situation.

These patterns are quite consistent throughout the table and seem to indicate that whites are relatively unaffected by their early unemployment experience while blacks are very much affected. I say "seem to indicate" because these results are only correlational; they do not prove cause and effect. Nevertheless, the figures seem powerfull and persuasive.

Why should we find these patterns? Once again, the answer is not known, but some plausible explanations are available. It may be, for example, that young blacks become in some sense more discouraged than whites. Or it is possible that employers hold a sporadic employment record against blacks but are inclined to overlook this for whites. Both explanations are possible, but a more persuasive explanation relies on our earlier discussions of job-finding techniques and how these differ by race. The data on consequences just presented seem to imply that whites in effect get more than one chance to "make it," but that for a black the chance, if any, comes once, and if available and taken the young black remains unusually stable and if missed then a worsening of employment prospects occurs. A pattern like this would result if whites relied on informal contacts--and hence could repeatedly receive help--while blacks were more dependent on formal institutions and thus both had their access rationed and were likely to be turned away in the face of a poor prior record. In this situation, blacks who did land a stable job, or a job with the potential for stability, would be much more likely to hold on to it and thus we would observe, as we do, early stability (or lack of unemployment) being more strongly correlated with later unemployment for blacks than whites.

This explanation is tentative and speculative and should be treated as such. However, the fact seems to remain that unemployment has more serious consequences for black than white teenagers and thus should add greater urgency to the formulation of policy.

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# THE EFFECTS OF CHILD LABOR LAWS ON YOUTH EMPLOYMENT

By: Daniel J. B. Mitchell & John Clapp*

## ABSTRACT

Child labor laws are a relatively neglected aspect of public policy towards youth employment. This is partly due to the difficulty in reducing the diverse state and federal laws in the child labor area into a quantifiable index. This study attempts to surmount the quantification barrier and analyze the employment effects of child labor laws.

Despite their name, child labor laws really regulate employment of teenagers, not children. They typically permit restricted labor force activity during ages fourteen to seventeen and do not apply at older age levels. Hence, comparisons of the employment behavior of those below age eighteen and those eighteen years or older should reflect the effects of the laws.

At approximately age eighteen, individuals typically graduate from high school. Thus, a general sample of young people is likely to be dominated by the effects of graduation, particularly in regard to the availability for work. To avoid confounding the graduation effect with the legislative effect, a sample of high school dropouts aged fourteen to twenty-one was drawn from the 1970 Census of Population. Comparisons of those younger than eighteen with those eighteen and above indicate that employment shifts toward sectors restricted by child labor at the eighteenth birthday. This shift suggest that the laws do restrict employment in selected sectors. A more tenuous analysis suggests that the laws have the effect of restricting total employment (as opposed to its pattern) for dropouts under age eighteen.

Arguments for child labor laws include the proposition that premature employment leads to "dead-end" jobs. Census data on occupational mobility of young people during 1965-1970 do suggest a considerable degree of immobility. Since dropouts tend to obtain lower status jobs, occupational immobility tends to keep them in such employment. However, the paper questions whether current public policy is an appropriate response to this phenomenon.

## INTRODUCTION

One hundred years ago, the normal activity of a teenager would have been work, not school. Today, it is assumed that the opposite should be true. Public policies, especially child labor laws and school leaving laws, reinforce this popular conception. Child labor laws restrict youth

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employment in certain occupations and industries. School leaving laws limit the availability of young people for work. This paper presents an empirical analysis of the employment impact of such legislation.

Both school leaving laws and child labor laws were the products of public reaction to perceived evils in the period from the late nineteenth century to the early twentieth. These laws have been periodically updated. But since the 1930s, they have received little critical attention. Despite this neglect, current concerns about youth employment problems must inevitably force a re-examination of the impacts of these programs.

Rethinking public policies is not an easy task; remaking public policies is even harder. Current legislation with regard to "child" labor --really teenage labor--could not be changed without creating some adverse effects.[1] Inevitably, there would be concern about the impact on the adult labor market of an increase in the supply in teenagers. But the fact that reform is complicated and raises potentially controversial issues is not reason for avoiding the needed review.

Indeed, it could be argued that the time is especially right for a re-appraisal. Historically, child labor laws and female protective labor laws were seen as part of a package; both women and children were seen as needing special state protection at the turn of the century. Although female protective labor laws seemed sacrosanct, the desexing of state labor codes--under the impetus of Title 7 of the Civil Rights Act of 1964--was accomplished smoothly.[2] The lessons from that episode should carry over into child labor law reform. Moreover, the recent decline in birth rates will reduce the proportion of teenagers in the working age population during the next two decades. Youths fourteen to seventeen accounted for

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1. This study does not concern itself with agriculture, where use of preteenage children is a source of controversy. The youth employment problems discussed below are basically urban problems.

2. See Raymond Munts and David G. Rice, "Women Workers: Protection or Equality?" Industrial and Labor Relations Review, vol. 24 (October 1970), pp. 3-13.

10.5% of the population of fourteen and above in 1970, but will account for only 6.8% of the population by 1990, a reduction of well over one third.[3] Thus, concerns about the impact of teenage employment on adult job opportunities should be lessening.

Recent studies on child labor laws have been rare. One unpublished study attempted to relate such laws to juvenile delinquency rates.[4] Another relied heavily on employer interviews.[5] The problem has been that the laws, which vary from state to state and between the states and the federal government, have seemed inherently resistant to quantitative analysis. When studying protective legislation, economists have tended to prefer analysis of the minimum wage and its impact on teenage employment, probably because the minimum wage can be expressed as a number. Ironically, state minimum wage provisions and child labor laws are usually found in the same labor codes. And, of course, federal child labor provisions are included in the Fair Labor Standards Act, the same statute

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3. The estimate for 1990 is from the Census Series II projection. Data from U.S. Bureau of the Census, *Statistical Abstract of the United States, 1976* (Washington: Government Printing Office, 1976), p.6.

4. Donald G. Woodworth, *The Effects of Laws Governing Youth Employment and School Attendance on Youth Offenses and Delinquency*, unpublished report by Stanford Research Institute prepared for the Office of Juvenile Delinquency and Youth Development, U.S. Department of Health, Education and Welfare, December 1965. (Report available from Educational Resources Information Center.)

5. National Committee on Employment of Youth, *The Transition from School to Work: A Study of Laws, Regulations and Practices Restricting Work Experience and Employment Opportunities for Youth*, unpublished report prepared for the U.S. Office of Education, June 1975. It might be noted that the committee which sponsored the report--which is critical of unwarranted legal restrictions on teenage employment--was formerly the National Child Labor Committee, the major forum for child labor law advocates in the early twentieth century. See Walter I. Trattner, *Crusade for the Children: A History of the National Child Labor Committee and Child Labor Reform in America*, (Chicago: Quadrangle Books, 1970).

which establishes the federal minimum wage and overtime provisions. Thus, a researcher interested in teenage minimum wage impacts cannot help but run across the child labor provisions in these various statutes.

It would be incorrect to suggest that contemporary observers have been unconcerned about the possible effects of child labor laws. But there have been relatively few specific proposals for reform. Most proposals have been quite general. A typical example is a recent book by Willard Wirtz, the former Labor Secretary. Wirtz states that:

> Federal and state prohibitions on persons under eighteen working in "hazardous occupations" are perhaps significantly involved (as a barrier to youth job opportunities). Although federal offices report that only about 5 percent of all employment is foreclosed to sixteen and seventeen-year-olds by such prohibitions, employers manifest a considerable concern about their scope ... It adds to this confusion that the types of employment allowed for fourteen and fifteen-year-olds are considerably more limited ... A comprehensive effort must be made to eliminate these misunderstandings.[6]

Wirtz makes two points. First, he suggests that the effect of child labor laws should be small--that only 5% of potential employment is affected. The difficulty with this estimate is that it ignores the fact that teenagers are not likely to enter many forms of employment, especially those requiring professional training and skills. What matters is the proportion of jobs which could reasonably be sources of employment and which are in fact restricted by law. For reasons to be discussed below, it is difficult to pinpoint precisely which occupations and industries are directly or indirectly affected by child labor laws. However, a sample of high school dropouts was assembled for analysis in this paper. It was found that roughly half the males and a fourth of the females in the sample worked in occupations and industries which appear most likely to be affected after they attain age 18. Since youngsters 18 years of age and older are no longer subject to child labor restrictions, the potential impact of the laws on younger workers appears larger than Wirtz's 5 percent estimate.

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[6] Willard Wirtz, The Boundless Resource: A Prospectus for an Education-Work Policy (Washington: New Republic Book Co., 1975), p. 60.

The second suggestion is that the problem could be alleviated by supplying more information to employers. In fact, there is no shortage of Labor Department pamphlets on the subject. The difficulty is that the assertion contained in such pamphlets--that the laws are basically simple to understand and obey--is false.[7] Instead, the laws are complicated, confusing, and overlapping.

## DEVELOPMENT OF CURRENT POLICY

Two legal questions surround the field of child protective legislation. First, at what age do individuals become responsible for their own behavior and choices? The long-term rise in living standards has gradually permitted this age to rise, and only recently has a countertrend of "children's rights" begun to develop.[8] Second, below the critical age of maturity, who will be the party responsible for making decisions for minors? Basically, the alternatives have been family and state. For questions of education and work, child labor reformers gradually pushed the age of maturity up to eighteen years. Furthermore, they advocated a major role for the state--rather than the family--in dealing with these issues.[9]

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[7] As an illustration, consider a recent Labor Department release designed to convince employers "that there is nothing complicated about the laws governing summer jobs." The release goes on for three pages describing federal laws, and then notes that employers had better check up on state laws as well. In fact, a perusal of the release is sufficient to convince the reader that the laws are complicated and that the simple way to avoid violating them is to avoid hiring teenagers in a number of broad industrial sectors. See "Elisburg Explains the Laws Applying to Summer Jobs for Teenagers," U.S. Department of Labor, Employment Standards Administration press release, June 12, 1977.

[8] Readings on the issue of children's rights can be found in Robert H. Bremner, ed., The Legal Rights of Children (New York: Arno Press, 1974). Recent litigation has centered on the rights of juvenile delinquents to due process procedures, the rights of dependent children in divorce cases, and rights of expression in student newspapers. The minimum voting age was lowered to eighteen years by the twenty-sixth amendment to the constitution. Some observers have claimed that the age of physical maturity, i.e., puberty, has been declining. On the last point, see John A. R. Wilson, et al, Psychological Foundations of Learning and Teaching, second edition (New York: McGraw-Hill, 1974), p. 302.

A complicated web of social and economic forces contributed to the rise of the child labor law movement. Industrialization brought about a rise in productivity which made it economically feasible to delay work until the late teenage years. As a consumption good, it might be expected that an increased demand for education would arise as living standards rose. From an investment viewpoint, industrialization appeared to create a wide disparity in wage differentials between skilled and unskilled workers.[10] To the extent that education provided a route into skilled employment, the demand for it could be expected to increase.

Industrialization also meant that child labor would take place in the context of an employer-employee relationship rather than a parent-child relationship, at least in urban areas. The conversion of child labor into a market transaction brought it to greater public attention. In contrast, reformers tended to romanticize the virtues of child labor on family farms --even on subsistence farms--despite some contrary evidence.[11] The fact

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9. An excellent compilation of documents relating to child labor, child labor laws, and related issues can be found in Robert H. Bremner, ed., Children and Youth in America: A Documentary History (Cambridge, Mass.: Harvard University Press, 1970-74).

10. See Harry Ober, "Occupational Wage Differentials, 1907-1947," Monthly Labor Review, vol. 67 (August 1948), pp. 127-34; and Paul G. Keat, "Long-Run Changes in Occupational Wage Structure, 1900-1956," Journal of Political Economy, vol. 68 (December 1960), pp. 584-600.

11. For more on this issue, see Daniel J. B. Mitchell, "A Furor Over Working Children and the Bureau of Labor," Monthly Labor Review, vol. 98 (October 1975), pp. 34-36.

that child labor laws are generally less strict in agriculture is partly due to this tendency.

Urbanization also weakened the importance of the family as a self-contained production unit.[12] The failure of the family to maintain control over children among the urban poor led to the phenomenon of "street children" who still have counterparts today throughout the less-developed world and in contemporary American city slums.[13] The visibility of such street children led inevitably to the view that "the public must in some measure take the place of those who ought to have been their natural guardians and protectors."[14] The groundwork was thus laid for state intervention in matters of employment and education, because the traditional family was viewed as an inadequate control.

Opinions on why parental authority was inadequate varied. Some reformers simply viewed poor parents as greedy exploiters of their own progeny. Others assumed that the seeming lack of parents' concern for their children's educational needs or for the perils of too-early labor force participation stemmed from the regrettable ignorance of foreign or rural immigrants into urban centers. Some also saw required public education as a method of weaning immigrant children from their alien cultures.[15]

As a result of pressure for child labor laws and schooling laws, most states by the turn of the century required education somewhere between ages seven or eight and fourteen to sixteen. In addition, some limitations on minimum wages and maximum hours of work for minors in industry were often provided. According to the 1900 Census, children in the age bracket ten to fifteen generally did not work but a minority of above 26% did, with agriculture being the most significant employer.[16] Recent evidence suggests that minimum schooling requirements in the nineteenth century tended to follow actual practice, i.e., those states with high educational attainment rates tended to raise their minimum school leaving ages. And it does appear that early state child labor laws restricted employment in certain sectors.[17]

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[12] Stuart M. Blumin, The Urban Threshold (Chicago: University of Chicago Press, 1976), p. 245.

[13] For a view from the early part of this century, see Edward N. Clopper, Child Labor in City Streets (New York: Macmillan, 1912). A contemporary description can be found in Claude Brown, The Children of Ham (New York: Stein and Day, 1976).

[14] A quote from officials of the New York House of Refuge, an agency which began to receive public grants in 1829. See Joseph M. Hawes, Children in Urban Society: Juvenile Delinquency in Nineteenth Century America (New York: Oxford University Press, 1971), p. 46.

[15] Bremner, Children and Youth, vol. 2, p. 605.

Within the child labor reform movement there developed a rift between those who wished to continue pressuring the states for stricter laws, and those who desired federal legislation. Ultimately, the movement united behind a push for a federal law. The initial outcome of this effort was the creation of the federal Children's Bureau of 1912, an agency which continues today, but no longer has responsibility in the child labor area.[18] Two attempts to pass federal statutes limiting child labor--one in 1916 and another in 1918--ran afoul of Supreme Court decisions declaring them unconstitutional. Child labor reformers then sought a constitutional amendment, an effort which failed after a fierce opposition campaign during the state ratification process.

The need for a special amendment ended with the change in stance of the Supreme Court toward such legislation in the late 1930s and early

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[16] Idem.

[17] See William M. Landes and Lewis C. Solmon, "Compulsory Schooling Legislation: An Economic Analysis of Law and Social Change in the Nineteenth Century," Journal of Economic History, vol. 32 (March 1972), pp. 54-91; and Allen R. Sanderson, "Child-Labor Legislation and the Labor Force Participation of Children" Journal of Economic History, vol. 34 (March 1974), pp. 297-99.

[18] See Robert H. Bremner, ed., The United States Children's Bureau, 1912-1972 (New York: Arno, 1974).

1940s. Congress passed the Fair Labor Standards Act (FLSA) in 1938, which contained child labor provisions along with minimum wages and overtime regulations. Since 1938, child labor has been regulated at both the state and federal levels.

## CURRENT REGULATIONS

Under the federal FLSA, illegal use of children or teenagers is defined as "oppressive child labor." Enterprises considered to be in interstate commerce under the act are prohibited from using oppressive child labor in the production of goods and services. Shipments of such goods ("hot goods") across state lines are also banned.

Outside agriculture, employment below age fourteen is basically prohibited, except for a few insignificant exceptions. Employment during ages fourteen and fifteen is forbidden in manufacturing, mining, and other areas found to be hazardous by the Secretary of Labor. At ages sixteen and seventeen, employment is regulated by means of "hazardous orders" issued by the Secretary of Labor which declare certain types of jobs to be detrimental to the health and well-being of teenage employees. These orders are most likely to affect manufacturing, mining, construction, transportation, and utilities, especially in occupations where contact with machinery, chemicals, explosives, and so forth is a possibility.

Special regulations issued by the Secretary of Labor limit hours of work for fourteen and fifteen-year-olds. When school is not in session, employees in this age bracket may not work more than eight hours per day nor more than forty hours per week. During the school year, the limits are eighteen hours per week and three hours per school day. In addition, other limits are applied on the hours during which work may take place.

Certain exceptions to the general rules apply for vocational programs. For example, provisions are made for work/study arrangements for fourteen and fifteen-years-olds under "work experience and career exploration programs." These WECEP programs are aimed at dropout-prone students, and provide work experience under supervision of local school authorities. Evidence from these programs suggests that the jobs provided have not in fact interfered with school performance; they may even have improved it.[19] There are also special exceptions for student learners and apprentices. However, a "Catch-22" applies, since apprenticeship programs often do not accept applicants below age eighteen, or require a high school diploma.[20]

An important element of federal policy is that the FLSA's section 18(a) permits state law to override the federal regulations, whenever the state law is stricter. In short, the provision applicable to an employer is always the stricter of the federal and state requirements. State laws vary considerably so that the importance of the FLSA compared with the state requirements will vary from state to state. In a given state, the FLSA may be applicable to certain youngsters and state law to others.[21]

It is not possible to review the various state laws.[22] However, it is worth pointing out that state laws may be scattered in various codes, a feature which makes it more difficult for employers to determine requirements. For example, in California it is necessary to consult the labor, education, and insurance codes to locate the relevant child requirements in the state. (The insurance code deals partly with workers' compensation laws, which have special features with regard to injury of illegally employed minors.) Certain sections of the administrative code are also applicable.

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[19] Dennis Roth and Ernst W. Stromsdorfer, An Analysis of the Educational and Economic Impact of the Work Experience and Career Exploration Program, unpublished working paper of the Office of the Assistant Secretary for Policy, Evaluation, and Research, U.S. Department of Labor, May 29, 1975.

[20] Other federal policies have an effect on teenage employment. In particular, although the FLSA permits some employment of 14-15 year olds, the Walsh-Healey Act--which applies to federal contractors--prohibits employment of youngsters under age sixteen.

[21] As an example, in California no provision is made for WECEP programs in the state law, so such programs cannot operate since state law overrides the federal. However, the FLSA is generally stricter for youngsters sixteen to seventeen-years-old than the state law.

[22] A somewhat dated summary of state child labor laws can be found in U.S. Bureau of Labor Standards, State Child Labor Standards, Bulletin 158, revised 1965 (Washington: Government Printing Office, 1965). Highlights of revisions in state laws can be found annually in the Monthly Labor Review.

State laws generally follow the FLSA in terms of the types of coverage. Typically, regulations vary at different age levels, with younger workers subject to stricter standards. Certain types of employment are restricted on the grounds of physical or moral hazard. Hours are limited to dovetail with school requirements. The main unifying point about both state and federal laws in this area is that application basically ceases at age eighteen. Thus, despite the difficulty in summarizing the details of the laws, comparisons of the labor market behavior of youngsters below age eighteen with that of those above that age can provide some indication of the impact of overall child labor policy.

## SECTORAL EMPLOYMENT IMPACTS OF CHILD LABOR LAWS

The dearth of professional studies on the impact of child labor laws leaves open the question of whether such laws have any impact at all. Perhaps, due to inadequate enforcement or a "natural" avoidance of modern-day teenagers of the types of jobs which are restricted, the laws do not influence labor-market behavior. In principle, the laws seek to shift teenage employment from certain sectors which are declared "off limits" to others which are considered to be more desirable. Hence, it is essential to examine available employment data to determine if such an employment-shifting effect can be detected. If it cannot, there is little point in considering the reform of laws which--for whatever reason--have no noticeable influence.

School dropouts are more likely than other teenagers to be affected by child labor laws. Dropouts are potentially available for full-time work while students are not. Hence, if the impact of child labor laws is detectable, the logical place to look is within a sample of high school dropouts.

The 1970 Census of Population "public use sample" computer tapes were used to provide a data base of individuals not enrolled in school, with educational attainment of less than a high school diploma. Residents of rural areas, who are not the focus of this study, were deleted. Analysis was confined to the West South Central, Mountain, and Pacific regions.[23] The limitation of the study to the western states was dictated by the availability of computerized data and computer time. It is believed, however, that the results discussed below have general applicability despite possible variations of state child labor laws across regions. This is because the federal regulations constitute a floor on state variation. Only individuals within the age range fourteen to twenty-one were included. This decision permitted observation on both sides of the eighteenth birthday while excluding individuals who were well into adulthood. A total of 7,764 individuals were selected, 48.5% of whom were male. Since the sample contains only one percent of those surveyed by the Census, the sample represents 776,400 persons in the western area. Within the sample, 2,724 persons were employed.

Initially, the goal was to determine whether child labor regulations had the anticipated effect of pushing employed dropouts in the sample away from sectors restricted by these laws. It is difficult to pinpoint industries and jobs which are specifically covered by child labor restrictions. But it is possible to designate certain industrial sectors and occupational classes which seem most prone to regulation. A reading of the laws suggests that the industries most likely to be affected are mining, construction, manufacturing, transportation, and utilities. The occupations most likely to be affected are blue collar wage-earning positions.[24] (Unpaid family employment is less strictly regulated.) Thus, one approach used was simply to define employment in the industries and occupations enumerated above as the "covered" sector, and all other employment as "uncovered."

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[23] For details on the public use sample, see U.S. Bureau of the Census, Public Use Samples of the Basic Records from the 1970 Census: Description and Technical Documentation (Washington: Government Printing Office, 1972). A comparison of the Western sample with published Census data for the entire urban United States suggests that no substantial demographic distortions were introduced by confining the sample to the western area. However, nonwhites tend to be somewhat underrepresented in the west compared with the entire country (17.4% of the dropout population aged fourteen to twenty-one versus 23.9%).

Since one goal of child labor legislation is to reduce exposure to unsafe working conditions, a second method of designating sectors likely to be heavily affected by child labor regulation was also utilized. As a byproduct of the Occupational Safety and Health Act of 1970, statistics on industrial work-injury rates are available. Thus, it is possible to determine accident rates according to detailed industry code and to isolate those industries with above average injuries per 100 full-time employee equivalents. Blue collar wage-earning employment in such industries was designated as "hazardous" and all other employment as "nonhazardous."[25]

If child labor laws had been constructed after detailed study of hazards, designation based on work injury rates would presumably capture sectors which were not available when most child labor provisions were drawn up. So despite the quantitative aspect of the hazardous/nonhazardous dichotomy, there is no reason to assume that it captures effects of child labor laws better than the simpler covered/uncovered classifications. In any case, there is considerable overlap between the covered and hazardous sectors.

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[24] Blue collar employment was defined as including the following Census occupation classes: craftsmen and kindred workers, operatives, laborers, and farmers.

[25] Industries were classified at roughly the three-digit S.I.C. level on the basis of recordable occupational injury and illness rates in 1973. (In a few cases of missing data, 1972 rates were used.) The mean rate for the private sector in 1973 was 11 cases per 100 full-time employee equivalents. Source: U.S. Bureau of Labor Statistics, Handbook of Labor Statistics 1975--Reference Edition (Washington: Government Printing Office, 1975). The use of the mean rate of injuries to classify industries is obviously arbitrary. It is possible that an alternative cutoff point would have produced different results. However, the general similarity of the results of the two alternative classification schemes described in the text suggests that the general conclusions would not vary with reasonable definitions of hazardous industries.

The first step in the analysis of employment patterns within the dropout sample was to determine whether the sectoral composition of employment shifts toward the covered or hazardous sectors at age eighteen. To make this determination, the sample was divided into employed individuals age fourteen to seventeen (ages regulated by child labor laws) and ages eighteen to twenty-one (ages not regulated by child labor laws). The sample was then subdivided into sex, race, and other demographic categories. Since the sample was relatively small, substantial disaggregation was not possible.

Table 1 summarizes these breakdowns for employed individuals. In all categories, except for nonwhite females, a higher proportion of those aged eighteen to twenty-one worked in the covered or hazardous sectors than those aged fourteen to seventeen. Of course, some of the cells are too small to permit much to be said about significance.[26] But in most of the larger cells, a statistically significant shift in employment patterns --in the direction child labor laws would lead one to expect--does occur. The shift seems to be greater for males than for females, probably reflecting a lower propensity of females at all ages to seek blue collar work in the restricted sectors.

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[26] An approximately normal distribution was used to test for statistical significance. The test statistic was defined as the ratio of the difference in sample proportions to the standard error of the difference. See Robert V. Hogg and Allen T. Craig, Introduction to Mathematical Statistics, third edition (New York: The MacMillan Company, 1970), pp. 201-2. For some of the smaller cells of Table 1, this test may be inaccurate. Most of the cells, however, contain at least fifty individuals, a point considered to be the cutoff for reasonable accuracy of the test.

These conclusions continue to hold when the sample is disaggregated along demographic lines. For example, there is no evidence of any differential on nonwhite females. (Of course, a large sample size for this group might be more revealing.) The results for nonwhite males are not substantially different than those for whites, and are less statistically significant. Young people not living with both parents and who are not themselves married with spouse present can be expected to have less adult guidance (or less adult responsibility) than other youngsters. But the differences between the sectoral employment composition of the older and younger individuals not living in husband-wife families are smaller and less significant than the differences for those who do live in such families. Similarly, since youth employment problems are often associated with large urban centers, it might be expected that those living in the larger cities would be more affected by child labor laws than those who do not. But the table indicates only slight differences in the results for larger and smaller urban areas.

Some evidence on the maturity effect can be found on Table 1. As was previously noted, comparisons of older and younger workers might be influenced by differences in maturity which alter employability in various sectors. One element of maturity at the age range in the sample is the tendency to be married. Only 14% of the males in the younger age group were married with spouse present, compared with 41% of the older males. The figures for females were 19% and 41%, respectively. However, even when the sample is broken down into single and married individuals, the gaps in the proportion employed in covered or hazardous jobs at the two age brackets remain. If marriage is important in determining these sectoral employment ratios--and it does seem to boost the ratios for males--it is still not sufficient to eliminate the marked shift in employment at age eighteen in the dropout sample.

There are, of course, other facets of maturity besides marriage. But whatever maturity entails, it seems unlikely that adulthood is a quality which is bestowed on an individual as a one-shot event at age eighteen. It seems more reasonable to assume that maturity accrues gradually. If

### TABLE 1
### EMPLOYMENT COMPOSITION OF EMPLOYED SCHOOL LEAVERS

| | Males | | | Females | | |
|---|---|---|---|---|---|---|
| Sector | 14-17 years | 18-21 years | Test Statistic | 14-17 years | 18-21 year | Test Statistic |
| | | | Total Sample | | | |
| Sample size | 399 | 1327 | - | 238 | 760 | - |
| Covered (%) | 32.3 | 49.1 | 6.16 | 13.5 | 23.7 | 3.79 |
| Hazardous (%) | 42.4 | 52.9 | 3.73 | 12.6 | 19.5 | 2.65 |
| | | | Whites | | | |
| Sample size | 353 | 1118 | - | 198 | 642 | - |
| Covered (%) | 32.0 | 49.3 | 5.96 | 12.1 | 24.8 | 4.40 |
| Hazardous (%) | 43.1 | 53.1 | 3.32 | 11.6 | 19.6 | 2.90 |
| | | | Nonwhites | | | |
| Sample size | 46 | 209 | - | 40 | 118 | - |
| Covered (%) | 34.8 | 47.8 | 1.67 | 20.0 | 17.8 | -.30 |
| Hazardous (%) | 37.0 | 51.7 | 1.86 | 17.5 | 18.6 | .16 |
| | | | White Blue Collar Workers (A) | | | |
| Sample size | 224 | 863 | - | 37 | 190 | - |
| Covered (%) | 39.7 | 57.4 | 4.81 | 48.6 | 72.1 | 2.66 |
| Hazardous (%) | 57.1 | 62.1 | 1.35 | 37.8 | 52.1 | 1.63 |
| | | | Nonwhite Blue Collar Workers (A) | | | |
| Sample size | 24 | 139 | - | 14 | 27 | - |
| Covered (%) | 37.5 | 65.5 | 2.62 | 50.0 | 74.1 | 1.53 |
| Hazardous (%) | 45.8 | 70.5 | 2.27 | 42.9 | 70.4 | 1.73 |
| | | | Living in Husband-Wife Families (B) | | | |
| Sample size | 291 | 977 | - | 159 | 535 | - |
| Covered (%) | 33.7 | 51.4 | 5.33 | 10.7 | 23.6 | 4.20 |
| Hazardous (%) | 44.3 | 56.1 | 3.55 | 10.7 | 18.9 | 2.75 |
| | | | Not Living in Husband-Wife Families (B) | | | |
| Sample size | 108 | 350 | - | 79 | 225 | - |
| Covered (%) | 28.9 | 42.6 | 2.92 | 19.0 | 24.0 | .95 |
| Hazardous (%) | 37.0 | 44.0 | 1.30 | 16.5 | 20.9 | .89 |
| | | | Single C | | | |
| Sample size | 342 | 784 | - | 192 | 446 | - |
| Covered (%) | 31.0 | 45.2 | 4.62 | 14.1 | 21.7 | 2.42 |
| Hazardous (%) | 41.2 | 40.0 | -.36 | 13.0 | 18.2 | 1.69 |

TABLE 1
(Cont'd)

| | Males | | | Females | | |
|---|---|---|---|---|---|---|
| Sector | 14-17 years | 18-21 years | Test Statistic | 14-17 years | 18-21 years | Test Statistic |
| | | | Married-Spouse Present | | | |
| Sample size | 57 | 543 | - | 46 | 314 | - |
| Covered (%) | 40.4 | 54.7 | 2.10 | 10.9 | 26.4 | 2.98 |
| Hazardous (%) | 49.1 | 59.9 | 1.54 | 10.9 | 21.3 | 2.04 |
| | | | Living in Smaller Urban Areas (D) | | | |
| Sample size | 177 | 579 | - | 108 | 326 | - |
| Covered (%) | 32.8 | 45.9 | 3.22 | 12.0 | 21.5 | 2.44 |
| Hazardous (%) | 42.9 | 51.1 | 1.92 | 13.0 | 17.8 | 1.25 |
| | | | Living in Larger Urban Areas (E) | | | |
| Sample size | 222 | 748 | - | 130 | 434 | - |
| Covered (%) | 32.0 | 51.5 | 5.38 | 14.6 | 25.3 | 2.87 |
| Hazardous (%) | 41.9 | 54.3 | 3.28 | 12.3 | 20.7 | 2.42 |

(A) See footnote 24 of text for occupations included.

(B) Living with both parents or married, spouse present.

(C) Includes never married as well as divorced, windowed, and separated.

(D) Urban areas of 500,000 population or less.

(E) Urban areas of population greater than 500,000.

Source: see text.

so, a better understanding of the maturity effect might be obtained from a more detailed age breakdown that was shown on Table 1.

Two problems arise when detailed age groupings are used. First, the sample size in any cell is reduced, decreasing the precision of the estimates. Second, comparisons of fourteen- to fifteen-year-olds with sixteen- to seventeen-year-olds are distorted by the differences in child labor laws which apply to these two age brackets. Both these considerations suggest that special attention should be paid to a comparison of eighteen- to nineteen-year-olds with twenty- to twenty-one-year-olds. The sample size is larger for these older persons than for the younger group and neither age is covered by child labor laws, except for possible "echo" effects for individuals not far beyond their eighteenth birthday.[27]

Table 2 shows that for males--both white and nonwhite-there is no significant shift in employment patterns toward restricted work between ages eighteen to nineteen and twenty to twenty-one. If maturity was pushing employment toward the restricted sectors at these age levels, a significant shift would be expected. The fact that none is found suggests that growing maturity by itself does not bias employment toward sectors restricted by child labor laws, at least at ages not far above the years when child labor laws apply. The gap between ages sixteen to seventeen and ages eighteen to nineteen, in contrast, is always significant and positive.

For females, the pattern is less clear. First, nonwhite females--when examined on Table 1--show no evidence of a shift toward restricted

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[27] Those who are just beyond their eighteenth birthdays may not have had sufficient time to change their sector of employment. Since these echo effects of child labor laws will reduce the significance of differences in the sectoral employment ratios between sixteen to seventeen-year-olds and eighteen to nineteen-year-olds, the analysis described is strengthened.

## TABLE 2

## EMPLOYMENT COMPARISON BY DETAILED AGE, RACE, AND SEX: ALL WORKERS

| | 14-15 years | 16-17 years | 18-19 years | 20-21 years |
|---|---|---|---|---|
| White Males (Number) | 80 | 273 | 530 | 588 |
| Covered (%) | 20.0 | 26.7 | 42.6 | 45.7 |
| | - | (1.29) | (4.63) | (1.04) |
| Hazardous (%) | 22.5 | 40.3 | 48.1 | 47.8 |
| | - | (3.22) | (2.12) | (-.10) |
| Nonwhite Males (Number) | 15 | 31 | 94 | 115 |
| Covered (%) | 26.7 | 16.1 | 40.4 | 46.1 |
| | - | (-.80) | (2.92) | (.83) |
| Hazardous (%) | 33.3 | 19.4 | 42.6 | 50.4 |
| | - | (-.99) | (2.65) | (1.13) |
| White Females (Number) | 47 | 151 | 288 | 354 |
| Covered (%) | 6.4 | 13.9 | 20.1 | 28.5 |
| | - | (1.64) | (1.69) | (2.50) |
| Hazardous (%) | 14.9 | 10.6 | 18.1 | 20.1 |
| | | (-.75) | (2.22) | (.64) |
| Nonwhite Females (Number) | 10 | 30 | 39 | 79 |
| Covered (%) | 30.0 | 16.7 | 20.5 | 16.4 |
| | - | (-.83) | (.40) | (-.53) |
| Hazardous (%) | 20.0 | 16.7 | 23.1 | 16.4 |
| | - | (-.23) | (.67) | (-.84) |
| Total Males (Number) | 95 | 304 | 624 | 703 |
| Covered (%) | 21.1 | 25.7 | 42.3 | 45.8 |
| | - | (.94) | (5.20) | (1.28) |
| Hazardous (%) | 24.2 | 38.2 | 47.3 | 48.2 |
| | - | (2.69) | (2.65) | (.33) |
| Total Females (Number) | 57 | 181 | 327 | 433 |
| Covered | 10.5 | 14.4 | 20.2 | 263 |
| | - | (.81) | (1.69) | (1.99) |
| Hazardous (%) | 15.8 | 11.6 | 18.7 | 20.1 |
| | - | (-.78) | (2.21) | (.48) |

Note: The test statistic in parentheses compares the figure above with figure in column to the left.

Source: See text.

employment at age eighteen. Hence, a search for a maturity effect among females must necessarily be confined to whites. Second, the data on white females between eighteen to nineteen ages and twenty to twenty-one suggest the existence of a maturity effect when the covered definition is used, but not when the hazardous definition is applied. That is, there is a statistically significant shift toward covered employment by white females between eighteen to nineteen and twenty to twenty-one, but not toward hazardous employment.

In short, it has been established that a statistically significant shift toward employment in sectors restricted by child labor laws occurs in a sample of western school dropouts between ages fourteen to eighteen and eighteen to twenty-one. For males, it can reasonably be inferred that the shift is due to the lapsing of these laws at age eighteen. Recall that the sample is composed entirely of high school dropouts. Thus, the males included in the sample did not experience the breaking point which the rest of the population encounters at approximately age 18: a high school graduation. Moreover, since there would presumably be a time lag between a person's eighteenth birthday and a resultant employment shift, the fact that a statistically significant shift is observed in the data lends support to the child labor law interpretation. Due to overlapping jurisdictions, there is no way of separating the state versus federal impact.

For females, the evidence is less clear. Among whites, the possibility that some undefined gradual maturity effect is causing the shift rather than a cessation of legal restriction cannot be dismissed. However, the suggestion that child labor laws affect males more than females is not surprising; the restricted types of employment have tended to be traditionally male. Of course, as women enter "male" occupations in greater numbers, this distinction could fade.

## OVERALL EMPLOYMENT IMPACTS OF CHILD LABOR LAWS

The evidence presented so far deals only with the mix of employment between sectors, not with the overall number employed. In principle, child labor laws could affect the mix and not the magnitude. However, it is also possible that the laws complicate the job search process for teenagers, thus resulting in fewer teenagers actually finding employment. Or, it could be that the laws "crowd" teenagers into certain sectors where the resulting depression of wages either is or becomes limited by a minimum wage floor. At such a floor, the number of jobs is inherently fixed, and teenage disemployment could result. Finally, it is possible that teenagers view the jobs in the unrestricted sectors as less desirable than restricted jobs. If so, labor force participation of teenagers might be reduced.

In exploring the possibility of an overall employment impact, the sample of western school dropouts is again of use, since the individuals included could be expected to be most sensitive to regulation by child labor laws. Even with this sample, it was felt that more could be learned by using employment rather than unemployment as the dependent variable. At the younger end of the labor market, the definition of unemployment creates well-known problems relating to discouragement and marginal attachment to the labor force. Other studies have found the employment-to-population ratio to be useful at this age range since its meaning is less ambiguous.[28] Thus, the calculations described below make use of this ratio.

Younger school leavers tend to have relatively low employment-to-population ratios when compared with older school leavers. The causes of this difference probably include the tendency of younger persons to be perceived as having lower productivity, to have less job experience, and to be under less pressure to seek work due to lack of dependents or income from family sources. Thus, the difference between the employment ratios for younger individuals below age eighteen and older ones should represent an upper-limit estimate of the overall child labor law effect. The estimate can be somewhat refined by confining the older comparison group to eighteen- to nineteen-year-olds, i.e., omitting those in the sample aged twenty to twenty-one. Even so, it should still exaggerate the child labor law impact.

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[28] See U.S. Bureau of Labor Statistics, Youth Employment and Minimum Wages (Washington: Government Printing Office, 1970), p. 43.

An alternative procedure is to make an explicit adjustment for maturity in explaining employment-to-population ratios at ages below eighteen years. The most obvious adjustment is to take account of the maturity effect observed between eighteen- to nineteen-year-olds and twenty- to twenty-one-year-olds, broken down by race and sex. Differences between these older groups can then be projected backwards onto sixteen- to seventeen-year-olds and fourteen- to fifteen-year-olds on a linear basis. Such adjusted ratios, when compared with the actual figures, can be expected to represent lower-limit estimates of the effects of child labor laws, since greater changes in life style are likely to take place for dropouts between eighteen or nineteen and twenty to twenty-one years than between fourteen or fifteen and sixteen to seventeen years. In particular, a greater tendency to marry, acquire dependents, or simply be "on one's own" is likely to be found between the older age brackets than between the younger ones.[29] However, it must be recognized that a linear adjustment is extremely crude and that the resulting estimates are best viewed as suggestive rather than definitive.

Table 3 presents the results of the two calculations. Shown is the employment "deficit," the difference between actual employment and employment that would have been expected in the age range fourteen to seventeen years based on employment-to-population ratios of eighteen- to nineteen-year-olds. The lower-limit estimates include a linear adjustment for

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[29] In 1970, the year of the Census, single men under twenty years had an overall labor force participation rate of 49.0%, compared with 95.5% for married men with spouse present at the same age. Married women under twenty with spouse present had a rate of 36.0%, not far below the single rate of 39.5%. Thus, marriage tends to raise the labor force activity of males drastically, and lower it for females somewhat. The propensity to become independent at this age level is likely to outweigh the somewhat depressing effect of marriage for females. Data from U.S. President, Employment and Training Report of the President, 1977 (Washington: Government Printing Office, 1977), Table B-2.

TABLE 3

EMPLOYMENT "DEFICIT" ESTIMATES FOR 14-17 YEAR OLD DROPOUTS

| | Lower-Limit Estimate | | Upper-Limit Estimate | |
|---|---|---|---|---|
| | Estimated Employment "Deficit" 1/ | "Deficit" as percent of non-employment 2/ | Estimated Employment "Deficit" 1/ | "Deficit" as percent of non-employment 2/ |
| White Males | -5,200 | 8.3% | -13,300 | 21.3% |
| White Females | -5,900 | 6.2% | -11,900 | 12.4% |
| Nonwhite Males | - | - | - 4,600 | 15.0% |
| Nonwhite Females | - | - | - 1,000 | 4.8% |
| Total | -11,000 | 5.3% | -30,800 | 14.7% |

[1/] Each individual in sample represents 100 persons. The "deficit" is the difference between actual employment and employment which would be expected based on employment ratios of older persons. See text for details.

[2/] Nonemployment consists of persons unemployed or not in the labor force.

Source: See text.

maturity; the upper-limit figures do not.[30] Taken literally, the figures suggest that 5-15% of urban school leavers aged fourteen to seventeeen years in the western area who are not employed would be employed in the absence of legal constraints. These estimates represent a range of 11,100 to 30,800 persons in the western area. Projected to the national level, the range would be on the order of 38,000 to 106,000.

It is unfortunate that the range of estimates is so wide. The authors would lean toward the lower-limit estimates as being more accurate, despite their exclusion of nonwhites. That females make up more than half of the lower-limit estimate and over 40% of the upper-limit estimate is suspicious. Nonwhite females did not seem to evidence a sectoral employment impact, but they are represented in the upper-limit deficit estimates. White females are represented in both upper- and lower-limit deficit estimates. As was noted in the previous section, evidence on the sectoral employment effect for this group was ambiguous. But it is possible that child labor laws could have a discouraging effect on total employment for females, even in the absence of a sectoral shift effect. The laws impose restrictions on employment in all sectors with regard to hours, working paper requirements, workers' compensation laws, and so on.

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30. For the upper-limit estimates, the difference between the employment to-population ratios for eighteen to nineteen-year-olds was compared with that for fourteen to fifteen and sixteen to seventeen-year-olds. The differences were multiplied by populations in the sample at these two age ranges to estimate the employment deficit. This calculation was made by race and sex. For the lower-limit estimates the eighteen to nineteen-year-old ratios were lowered by a maturity factor to obtain the "expected" rates for sixteen to seventeen-year-olds. The derived rate was again lowered by the maturity factor to obtain the expected rate for fourteen to fifteen-year-olds. As an example, for white males the employment ratio for eighteen to nineteen-year-olds was 50.0%. This was 6.3 percentage points below the ratio for twenty to twenty-one-year-olds. Thus, the maturity factor was set at 6.3. The expected ratio was set for sixteen to seventeen-year-olds as 50.0 - 6.3 = 43.7%. The expected ratio for fourteen to fifteen-year-olds was set at 43.7 - 6.3 = 37.4%. Actual ratios for these two age ranges were, respectively, 40.9% and 26.8%. Hence, a deficit in employment is indicated. For nonwhites, the maturity-adjustment technique tended to underestimate the employment ratios, so no deficit is estimated for them.

The analysis of the overall employment effect is certainly tenuous. But it does suggest that child labor laws do have an employment-discouraging impact, at least on certain dropouts. Given the magnitudes estimated, relaxing the laws obviously will not solve the entire youth employment problem. But it does appear that costs are imposed by current policy. The next section critically examines some of the benefits that the laws are said to engender.

## BENEFITS OF CHILD LABOR LAWS

Arguments for child labor legislation have usually centered around three aspects of youth employment. First, premature work was believed to endanger a child's health and safety. Second, early work was said to be a cause of delinquency, while school was held to repress delinquency. Third, it was argued that premature work would limit a child's education and the benefits derived therefrom. In particular, it was believed that premature entry into employment would lead to a lifetime of "dead-end" jobs due to insufficient education and opportunity for training. That is, a dropout's first job would be typical of what he or she could expect over a lifetime. These issues are discussed below, and some longitudinal data from the 1970 Census are offered on the issue of a lock-in effect with regard to dead end jobs.

### (i.) Safety

There is little of an empirical nature that can be said on the safety issue. Contemporary child labor laws really regulate the employment of teenagers, not children. Thus, the older arguments about stunted growth no longer apply. A key issue is whether teenagers--if they worked in currently restricted areas--would have higher accident rates than adults. This question cannot be answered from standard sources of labor-market data. It might be inferred from automobile accident records that teenagers are less careful than adults. However, unless a case can be made that on-the-job accident rates would be higher, it is hard to see the justification for special arrangements for teenagers--particularly older ones--in the face of federal job-safety requirements for all workers under the Occupational Health and Safety Act. Perhaps some comfort can be taken from the finding above that the hazardous and covered sectors heavily overlap. To the extent that a special rationale for teenage safety versus adult safety can be adduced--e.g., the lifetime cost of a permanent injury is likely to be higher for a younger person with more years in the labor market than an older person--it is reassuring to find that child labor laws do in fact tend to cover relatively hazardous industries.

(ii.) Work, School, and Delinquency

Although it may seem surprising to contemporary researchers, in the early part of this century, employment was seen as a cause of delinquency, not a cure. The so-called "street trades"--shoeshine boys, newsboys, and messengers--were viewed as little more than excuses to stand around on street corners and get into trouble. A major study by the old Bureau of Labor demonstrated that delinquents were more likely to have been employed than nondelinquents, and imputed a causal relationship running from work to crime.[31] A modern analyst would be more likely to assume that work and delinquency were both negatively associated with family income in the Bureau of Labor's sample.

The assumption that school reduces delinquency has been questioned in recent literature. The actual effect appears to depend critically on the type of experience a youngster is having in school. Failure in school can be an alienating experience which increases the propensity for crime. To the extent that dropouts can make a reasonable accommodation to adult norms, while at the same time meeting some of their own aspirations, their delinquent tendencies can be reduced.[32] Improved access to employment is an obvious route to both socialization and achievement of aspirations.[33]

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[31] U.S. Senate Documents, 61st Congress, 2nd Session, Woman and Child Wage-Earners in the United States, vol. 8, "Juvenile Delinquency and its Relation to Employment" (Washington: Government Printing Office, 1911).

[32] Delbert S. Elliott and Harwin L. Voss, Delinquency and Dropout (Toronto: Lexington Books, 1974), p. 60.

### (iii.) Education and the Lock-in Effect

The correlation between income and education is so well known that there is little point in commenting on it. Nor can the long-standing debate as to whether added education truly is the cause of higher income be discussed here. But one point is worth noting. The gross correlation between education and income seems to be heavily dependent on the relationship between education and occupation. Education appears to be an entry-way into better occupations--in terms of pay and other features. Once the occupational effect is removed, the variance in income explained by education is reduced. It is easy to find detailed occupational classifications in which dropouts have only minor wage disadvantage--if any--in full-time earnings relative to high school graduates.[34]

Still, it cannot be denied that on the average, dropouts fare more poorly than graduates. Even substitute forms of education do not seem to close the gap. For example, dropouts with vocational training enjoy a

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33. Some sociologists have emphasized blocked paths to achievement of aspirations as a cause of delinquency. Put simply, if legitimate paths are closed, illegitimate paths are more likely to be chosen. See Richard A. Cloward and Lloyd E. Ohlin, Delinquency and Opportunity (Glencoe, Ill.: Free Press, 1969). Economists have also pursued this framework and find evidence for it. See Belton M. Fleisher, "The Effect of Unemployment on Delinquency," Journal of Political Economy, vol. 71 (December 1963), pp. 543-53, for evidence that unemployment and delinquency are positively correlated.

34. On the basis of median earnings for white males twenty-five years to thirty-four years of age working fifty to fifty-two weeks per year, twenty-two occupational classifications can be identified in which dropouts have an earnings disadvantage of less than 3%. See U.S. Bureau of the Census, Earnings by Occupation and Education, PC(2)-88 (Washington: Government Printing Office, 1973), Table 1. Dropouts are defined as those with one to three years of high school; graduates are those with no college attainment. Occupations include police officers, cashiers, insurance sales workers, bus drivers, and bank officials.

larger wage premium relative to untrained dropouts than trained graduates do relative to untrained graduates. But untrained graduates still enjoy higher average earnings than trained dropouts.[35]

The original child labor reformers saw great perils in premature employment. And, today, fears that early entry into "dead-end" jobs would lock teenagers into such jobs during their entire careers remain an important source of support for current child labor restrictions. It is therefore important to examine the available data for evidence of a "locked-in" effect. The evidence described below does suggest that such an effect may exist. However, as will be noted at the end of this section, this finding does not imply that child labor laws are the optimum response.

To deal with the lock-in issue, a second sample was drawn from the public user sample of the 1970 Census of Population for the western area. In the 1970 Census, some respondents were asked about their current employment and their employment five years before (in 1965). Thus, it is possible to trace occupational mobility patterns across a five-year period. Since child labor laws would affect the patterns of anyone who was under eighteen years in 1965, the sample was confined to individuals at least eighteen but not more than twenty-five years of age in 1965. Again, it was hoped that the behavior of older individuals would provide some guide to the behavior that younger persons would exhibit in the absence of legal constraints. The sample was further confined to urban males with educational attainment of at least nine, but not more than

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35. A twenty-five to thirty-four-year-old male dropout with vocational training had median earnings in 1969 which were 17% higher than a dropout without training. A high school graduate with no college attainment with training had only a 7% premium above an untrained graduate. But a trained dropout earned 7% less than an untrained graduate. See U.S. Bureau of the Census, Vocational Training, PC(2)-5C (Washington: Government Printing Office, 1973), Table 10.

twelve years who had been employed both in 1965 and 1970. A total of 5,434 individuals were included, representing a western population of 543,400.

For purposes of this study, occupations were ranked by average male full-time earnings at all age levels. The all-age average appeared to be a better guide to lifetime earnings than the earnings of younger workers. A total of 102 Census occupations was used to classify employment. A move to a lower-ranked occupation was considered a deterioration. Since no two occupations had exactly the same earnings, no change in status was synonymous with no change in occupation.

The lock-in effect would be benign if it applied only to high wage occupations. Thus, concern centers on the possibility of being locked into low wage positions. Obviously, the cutoffs for defining low and high wage jobs are inherently arbitrary. As a matter of convenience, the sample was divided roughly into thirds on the basis of a ranking of individuals by their occupation's average wages. The bottom third fell in occupations with average earnings below $6,371, while the top third was in to occupations with average above $7,795. These classifications were then used to categorize the initial type of job of each individual.

Each person in the sample experienced one of two basic possibilities during 1965-1970. Either he remained in his 1965 occupation or he moved to one of the other 101 occupations. If an individual moved, the shift is described as an improvement on Table 4 if the move was to a higher wage occupation. Thus, a move from an occupation with a wage of $5,000 to one with a wage of $5,001 was classified as an improvement although both are low-wage occupations. Of course, a move from a $5,000 occupation to an $8,000 occupation--involving a jump from a low-wage to a high-wage occupation--would also be classified as an improvement. Similarly, a deterioration is recorded on Table 4 whether the decline was large enough to drop from high to middle wage, middle to low wage, or simply involved a dollar drop within the high, middle, or low wage classes.

There are few surprises regarding the qualitative aspects of the results. It would be expected that there would be more upward mobility from low wage jobs than from high wage jobs, and *vice verse* for downward mo-

TABLE 4

MALE EDUCATION AND OCCUPATIONAL MOBILITY, 1965 - 1970

| | High School Dropouts | | | | High School Graduates | | | |
|---|---|---|---|---|---|---|---|---|
| Change in Status, 1965-1970 | High Wage | Middle Wage | Low Wage | All | High Wage | Middle Wage | Low Wage | All |
| Improvement | 9.0% | 35.4% | 54.0% | 36.5% | 13.0% | 37.7% | 64.6% | 37.7 |
| No Change | 51.9 | 41.0 | 37.8 | 42.3 | 54.5 | 41.7 | 29.2 | 43.1 |
| Deterioration | 39.1 | 23.6 | 8.2 | 21.2 | 32.5 | 20.6 | 6.2 | 20.2 |
| Total | 100.0 | 100.0 | 100.00 | 100.0 | 100.0 | 100.0 | 100.0 | 100.0 |
| Number | 432 | 669 | 717 | 1,818 | 1,213 | 1,292 | 1,111 | 3,616 |

Note: See text for source of data and definitions.

bility. This is partly because it is hard to go down from the bottom or up from the top, and partly because of the nature of the tabulation. If it is assumed that there is a random error present in occupational placement at any point in time, at a later date individuals will be found to move toward their "proper" occupations, where "proper" is defined in terms of characteristics affecting productivity and employability.

Since absolute dollar definitions are being used for defining low wage and high wage, and since dropouts are known to have lower average wages than graduates, the comparison of mobility across the two groups also does not produce qualitatively surprising results. It would be expected that with the same definitions, fewer dropouts than graduates would show improvement in occupational status from low wage occupations and more dropouts than graduates would show downward mobility from high wage occupations. This is because graduates will have a greater tendency to "belong" in the high wage occupations than dropouts, while the reverse will be true for low-wage occupations.

Table 4 confirms these various expectations. What is surprising is that both graduates and dropouts show a high rate of occupational immobility--no change in status over the period--despite the relative youthfulness of the sample.[36] Apparently, the high degree of mobility which is often associated with youth relates more to changes of employer than changes of occupation. The table shows that 42.3% of the dropouts and 42.1% of the graduates experienced no change in occupation during the five-year period.

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[36] According to national data for males of all age groups, the overall occupational immobility rate was about 60%. Thus, the immobility figures on Table 4 for the younger groups are significantly lower, as expected. See Dixie Sommers and Alan Eck, "Occupational Mobility in the American Labor Force," Monthly Labor Review, vol. 100 (January 1977), p. 9. Note that the figures in the Sommers and Eck paper must be adjusted to exclude individuals not working or who died during 1965-1970 to obtain estimates comparable to those discussed in the text.

These rates probably understate the mobility rates that would apply to teenagers below age eighteen in the absence of legal constraints, since even within the sample, the younger workers are more mobile than the older.[37] But the fears of child labor reformers of a lock-in effect do find some confirmation on the table.

If it is granted that the fears were well-founded, it is still unclear that the policies chosen in response were optimal. These policies both require full-time attendance at school until some minimum age (usually fourteen to sixteen) and limit the job opportunities available to dropouts under age eighteen. Both sets of policies provide incentives to complete high school. Assume for the moment that the laws have the desired effect of keeping teenagers in school who would otherwise drop out. At best, the laws would prevent the dropping out of students on the margin of the dropout decision. This marginal effect would be achieved by imposing costs on those who drop out despite the reverse incentives. Only about three-fourths of a high school graduating class finish their diplomas "on time." So the minority who bear costs is not an insignificant fraction of the nation's teenagers.[38]

## IMPLICATIONS

Despite the inherent difficulties in exploring the effects and underlying rationale of statutes as diverse as child labor laws, this study has

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37. At ages in 1970 of twenty-three to twenty-four, twenty-five to twenty-six, twenty-seven to twenty-eight, and twenty-nine to thirty years, the proportions of dropouts experiencing no mobility were, respectively, 33.5%, 37.5%, 43.9%, and 50.1%. For graduates the figures were 32.3%, 37.6%, 42.9% and 51.2%. The proportions of dropouts experiencing upward mobility were 43.0%, 40.6%, 35.3%, and 30.2%. For graduates, these proportions were 44.1%, 40.3%, 38.3%, and 31.0%. Thus, immobility rises with age, and upward mobility declines.

38. About 74% of the class which entered the fifth grade in 1966 graduated "on time" in 1974. See U.S. Bureau of the Census, Statistical Abstract of the United States 1976 (Washington: Government Printing Office, 1976), p. 140.

demonstrated that statistical investigations are possible. However, it has also demonstrated the limitations of such explorations, when confined to Census or similar data sources. The effects of the laws are detectable in Census data, but it would be difficult to carry out alternative policy simulations based on the available statistics.

Further information on the effects of child labor laws could best be determined through experimental techniques. It would be interesting to relax the laws in particular areas and observe the results. Unfortunately, the current arrangement whereby the applicable law is always the stricter of the federal or state statutes makes such experimentation difficult. A step toward experimentation and ultimate reform would be repeal of that portion of Section 18(a) of the FLSA dealing with child labor.[39] Such repeal would put child labor policy for all employment under the FLSA exclusively in federal hands. Thereafter, changes in policy would not require the coordination of diverse state laws.

Finally, researchers in the field of youth employment problems ought not to neglect the impact of child labor and related laws, despite the difficulty in quantification. It does appear that the impact is to push teenage employment--at least among dropouts--into sectors whose virtues in terms of long-term job prospects are questionable. In addition, they may well tend to discourage employment of some teenagers altogether.

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39. Section 18(a) permits the states to override the FLSA's child labor provisions with stricter regulations.

2.7

# YOUTH LABOR MARKETS AND THE MILITARY

By: Richard V.L. Cooper

## ABSTRACT

This paper outlines the effects that the military has on youth labor force participation and the youth job market, and concludes that the military's demand for labor is an important determinant of both the size and composition of the youth labor force. Changes in the military's demand for labor can have significant effects on the youth labor force, and other variables affecting American youth. The military also exerts a major influence on the supply-side behavior of the youth labor force. Of most significance for the civilian labor market is the human capital that former service members bring back when they rejoin the civilian work force. Thus youth unemployment rates ought to be defined in terms of the total labor force, not just in terms of the civilian labor force. Developing such appropriate measures of youth unemployment could lead to more informed policy decisions.

## INTRODUCTION

Youth unemployment has become an increasingly important problem in recent years. Youth unemployment rates have averaged nearly 20% since 1974, and have run as high as 40% in some segments of the youth labor market (e.g., black teenagers). Developing and implementing solutions to the youth unemployment problem has therefore become a major concern throughout the policymaking community.

Before the causes of and possible solutions to the youth unemployment problem can be identified, however, it is both important and necessary to develop a better understanding of the youth labor market--specifically, youth labor force participation and the youth job market. Because the military plays such an important role in the youth labor market, the purpose of this paper is to highlight some of the effects that the military has on youth labor force participation and youth job market. As such, this paper is intended simply to provide an overview for the labor economist not too familiar with the military. That is, it is not intended to break new ground--either theoretically or empirically--but rather to put into a common framework many of the issues addressed separately by both labor economists and military manpower analysts.

The next section of this paper examines briefly the effect of the military's demand for labor on the youth job market. The third section then turns to focus on the role of the military in youth labor supply and human capital development. The last section examines the implications of the military's role in the youth labor market for the development of meaningful labor statistics.

## THE MILITARY'S DEMAND FOR LABOR AND THE YOUTH JOB MARKET

Because the military is such a major claimant of the nation's resources, and of youth labor in particular, it is useful to begin by discussing the military's demand for labor. Whether the military enters the youth labor market through traditional market allocating mechanisms (e.g., wages and other inducements to join) or through nonmarket allocating mechanisms (e.g., the draft), the military demand for labor can have important effects on the size and composition of the youth labor force. That is, the youth labor force is shaped in significant ways by both the military's aggregate demand for labor and the more specific policies that govern the use of military personnel during their service careers.

The discussion below therefore centers on the demand for military labor. If, as is not the case, the military's demand for labor made up, say, 0.1 percent of the labor force, then the subject might be interesting, but of only academic interest. If, as is actually the case, the military makes up a significant share of the youth labor force--in the neighborhood of 10% to 20%--then this question of demand becomes of particular policy importance. To the extent that labor statistics are not designed to measure the military's effect on either the size or composition of the youth labor force, these statistics are accordingly less useful for policy purposes.

Recognizing the importance of the demand side of the equation, the discussion below focuses first on the overall size of the military, and then turns to the impact of the military on the youth labor market in particular.

## Size of the Military

With its four to five million employees, depending on who is counted, and its approximately $400 billion worth of land and capital in fiscal 1976, the Department of Defense is the single largest employer of resources in the nation. The military's capital stock consists not only of such obviously military items as tanks, ships, and aircraft, but also the more mundane items such as forklift trucks, buildings, desks and file drawers, and so forth.

The military's labor force includes about 2.1 million active duty uniformed personnel, about 1 million reservists (i.e., the so-called "weekend warriors"), 1 million direct-hire civilian personnel (of which about 600,000 are in white collar occupations, while the remaining 400,000 are in blue collar jobs), about 100,000 indirect--hire civilian personnel, 500,000 contract-hire civilian personnel, and about 250,000 nonappropriated fund employees.[1]

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[1] "Direct-hire" civilians are those civilian employees maintained directly on the defense payroll. "Indirect-hires" are those foreign and nationals working on U.S. installations abroad who are formally employed by the host nation, but whose costs are actually paid by the U.S. military through a reimbursement program. "Contract hires" are those individuals who, through actually employed by civilian firms, perform contract services for the military such as aircraft maintenance, janitorial services, and kitchen duties, among others. (Contract hires do not, however, include those civilian workers engaged in the production of equipment and construction ultimately purchased by the military.) "Nonappropriated fund" civilian personnel are individuals who are paid out of funds not budgeted out of Congressionally appropriated funds. These are largely employees of military commissaries and post exchanges who are paid out of the funds generated by the sale of goods and services.

For almost all of the civilian personnel, however, there is little difference between their employer and regular civilian employers.[2] What is unique about the military, though, are military personnel. For the most part, the discussion in this paper will focus on active duty personnel, but it is important to recognize that the nearly 1 million reservists represent an important type of second job holding, and need to be examined accordingly.[3]

Even when we focus only on military personnel, Figure 1 makes it clear that military personnel comprise a significant portion of the U.S. male labor force. In the immediate post-World War II period, male military personnel made up about 3.5% of the total male work force, but jumped to nearly 7.5% during the Korean conflict. Between the Korean and Vietnam wars, male military personnel made up between 5% and 6% of the U.S. male labor force. After jumping up to more than 6.5% during the Vietnam war, the proportion of the U.S. male work force in the military has declined to between 3.5% and 4% during the past several years. Thus, although not a dominant factor in the male work force, the military has nonetheless maintained a significant share of the labor force in its ranks during the entire post-World War II period.

## The Youth Labor Market

Because the military maintains a "closed" personnel system, the foregoing understates in an important way the impact of the military

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[2] There are some "dual-hatted" civilians, primarily maintenance technicians, who though employed as civilians, are also members of the reserve forces.

[3] For a discussion of moonlighting and the reserves, see Bernard D. Rostker and Robert Shishko, Air Reserve Personnel Study: Volume II. The Air Reserve Forces and the Economics of Secondary Labor Market Participation (Santa Monica, Ca.: The Rand Corporation, 1973); and Robert Shishko and Bernard D. Rostker, "The Economics of Multiple Job Holding," American Economic Review, vol. 66 (June 1976).

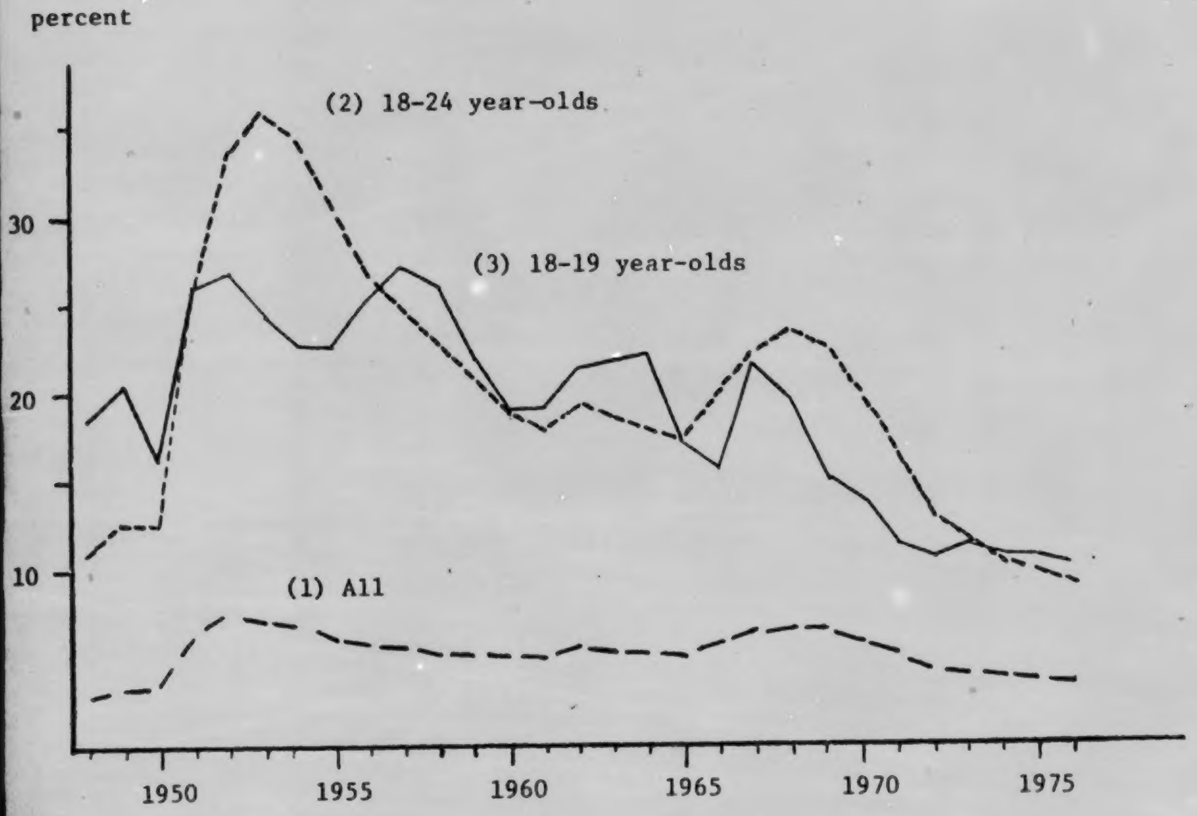

Fig. 1 -- Number of Male Military Personnel Relative to the Relevant Male Labor Force (percent): (1) All, (2) 18-24 year-olds and (3) 18-19 year-olds.

on the youth labor market. That is, the military maintains an "up through the ranks" personnel system, with little lateral entry. This means, then, that the military's major influence on labor markets is at the entry point, typically the crop of recent college graduates for the officer corps, and the recent crop of high school graduates for the enlisted ranks (although about 35% of enlisted recruits are non-high school graduates). As a result, about 90% of all enlisted personnel join the military between the ages of seventeen and twenty years old.[4]

The implications of the closed military personnel system for the youth labor market can be seen in Figure 2, which shows that between the 1950s and mid-1960s roughly half of all young men reaching military age served in the military. By the mid- to late-1970s, however, declining military force sizes and a substantially larger youth cohort meant that only about one out of every five young men would serve in the military at some time during his life.[5]

A somewhat different perspective on the effect of the military's demand for labor on the youth labor market can be seen in Figure 1 shown earlier. Specifically, Figure 1 also shows the proportions of the eighteen- to nineteen-year-old youth labor force and eighteen- to twenty-four-year-old labor force employed by the military. According to either of these measures, we see that between 20% and 35% of the youth labor force was employed by the military from the time of the Korean buildup through the Vietnam war. Only since the end of

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[4] Because of the "oldest first" draft policy of the 1950s and 1960s, significant numbers of personnel then entered the military in their mid-twenties.

[5] In fact, these demographic trends, more than anything else, were responsible for the demise of the draft. That is, the growing population base not only created enormous inequities (since a small few would have to bear the "burden" of military service, while the vast majority could escape serving), but also made it possible to attract sufficient numbers of volunteers without the threat of a draft.

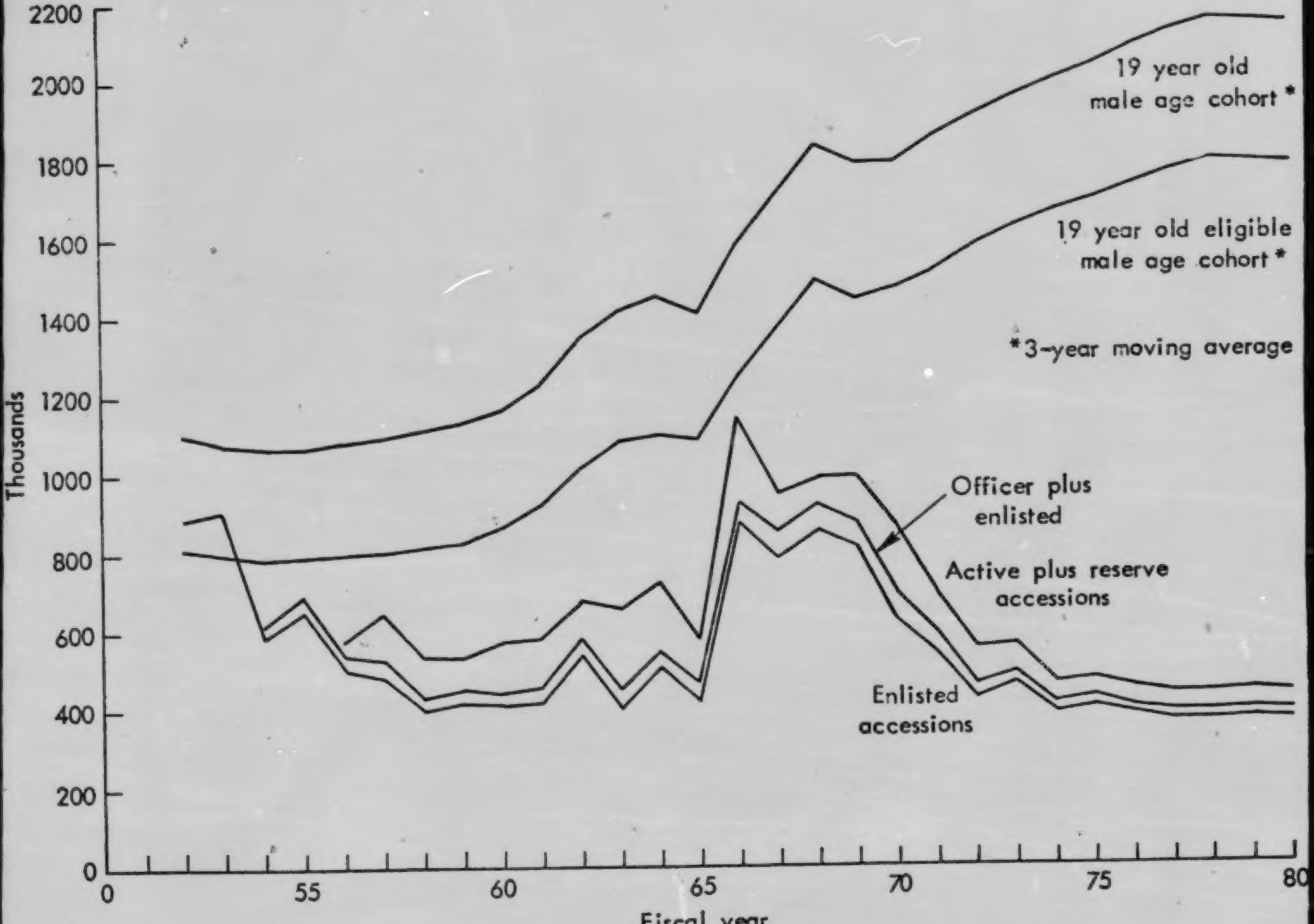

Fig. 2 — Military manpower procurement and population size

the Vietnam war, with the corresponding reduction in military force strengths and simultaneous increase in the youth population cohort, has the proportion of the youth cohort employed by the military dropped significantly--at its current level, to about 10%.[6] Thus, no matter how we measure it, the military is an important, and in some cases the dominant, player in the youth labor market. Accordingly, policy changes affecting the numbers of young men entering the military--and/or the nature of their military service--can have a significant impact on the size and composition of the youth labor force.

In addition to the quantitative side of demand, there is an important qualitative aspect to the military's participation in the youth labor market. Specifically, the military uses a variety of criteria to screen potential applicants for enlistment. The individual must first take a mental aptitude test, the results of which are used to clasify the individual into one of five so-called mental categories (with Mental Category I representing the top 7% of the population and Category V representing the bottom 10%). Those classified into Mental Category V are legally ineligible to serve. Others ineligible include those who fail to pass the medical examination, as well as those who fail to meet certain other criteria such as a check of police records, talks with high school counselors, and so forth. Overall, about 40 out of every 100 applicants for enlistment are rejected outright. Moreover, of the remainder, the military only allows some Mental Category IV (i.e., below average) and high school dropouts to join.

The end result of supply behavior (on the part of the individual) and this demand behavior (on the part of the military) can be seen in Tables 1 and 2, which compare the mental aptitude and educational

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[6] Note that these measures of the military's penetration exclude members of the reserve forces.

## TABLE 1.

## DISTRIBUTION OF ENLISTED ACCESSIONS AND THE GENERAL 18 TO 21 YEAR-OLD MALE POPULATION BY MENTAL CATEGORY (percent)

| Mental Category | Draft | AVF | US Population: Male 18-21 Year-Olds | | | |
|---|---|---|---|---|---|---|
| | | | All | | Non-College | |
| | | | All | Non-V | All | Non-V |
| I | 6 | 3 | 7 | 8 | 2 | 2 |
| II | 31 | 32 | 28 | 31 | 22 | 25 |
| III | 43 | 57 | 34 | 38 | 39 | 45 |
| IV | 19 | 8 | 21 | 23 | 24 | 28 |
| V | -- | -- | 10 | -- | 13 | -- |
| TOTAL | 100 | 100 | 100 | 100 | 100 | 100 |

Source: Office, Assistant Secretary of Defense (Manpower and Reserve Affairs)

## TABLE 2.

## EDUCATIONAL ATTAINMENT OF ENLISTED ACCESSIONS AND THE U.S. MALE POPULATION (percent)

| Maximum Educational Attainment | Enlisted Accessions[a] | | U.S. Male Population[b] | | | |
|---|---|---|---|---|---|---|
| | Draft | AVF | All 18-22 | Not in School 18-24 | Not in School 18-21 | Blue Collar 25-44 |
| College Grad. | 3 | 1 | 8 | 7 | 1 | 3 |
| Some College | 13 | 5 | 26 | 13 | 12 | 12 |
| High School Grad. | 54 | 59 | 41 | 46 | 49 | 48 |
| Some High School | 26 | 35 | 19 | 22 | 27 | 21 |
| Elementary | 4 | 1 | 6 | 11 | 12 | 16 |

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[a] Source: Office, Assistant Secretary of Defense (Manpower and Reserve Affairs)

[b] Source: U.S. Bureau of the Census and U.S. Bureau of Labor Statistics.

[c] Includes GEDs--i.e., those who have passed a general high school equivalency test, but who do not possess a high school diploma.

attainment, respectively, of military recruits with their civilian counterparts.[7] In general, these comparisons show that the military takes in a reasonably representative sample of the nation's youth. In terms of the nation's policy toward youth and youth unemployment Tables 1 and 2 establish the important point that the military does not draw narrowly from any one segment of society;[8] rather, the military plays a significant role in almost all segments of the male youth labor force.

Finally, the discussion of the military's participation in the youth labor market would not be complete without giving consideration to two special issues: the use of women in the military and the racial composition of new recruits. Although participation of women in the armed forces was limited by law to no more than 2% of military personnel strengths prior to 1972, these restrictions are now largely gone (the only major remaining restriction being that women are not to be employed in "combat," though the defination of combat is in the process of change). As a result, participation of women in the armed forces has increased significantly over the past five years, up to more than 5% of total military personnel today--that is, currently there are about 110,000 women in the military. Further increases are planned, such that women in the military should number between 150,000 and 200,000 by the 1980s. Moreover, women are being used in a variety of "nontraditional" jobs. That is, whereas women were once limited mostly to certain medical and administrative occupations, today they are entering a variety of occupational areas such as truck driver, aircraft mechanic and, in some cases, combat support. Thus, the military, which once only had a minor role in the female labor market, is now taking a much more active role and will continue to do so in the future.

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[7] That is, theory tells us that supply behavior should lead to fewer (than a random sample of) very high mental aptitude individuals joining the enlisted ranks, since the enlisted ranks correspond more or less with blue collar occupations and since these very high mental aptitude youth are more likely to attend college. On the demand side, the military limits (and in some cases excludes outright) the numbers of below average mental aptitude and non-high school graduates, so the bottom end of the mental aptitude and educational attainment spectra will also tend to be "underrepresented." Thus, the enlisted ranks of the military would be expected to have proportionately fewer members from the very top and bottom ends of the mental aptitude spectrum or from those with post-secondary and no secondary education. (Commissioned officers, on the other hand, are drawn exclusively from the college graduate population.) Note, however, from Tables 1 and 2 that enlisted recruits are quite representative of the upper end of the noncollege civilian population--i.e., those individuals corresponding most closely with enlisted occupations.

[8] Other work by the author shows that the military draws a reasonably representative sample of American youth in other dimensions as well, such as region of origin and socio-economic background. See Richard V.L. Cooper, Military Manpower and the All-Volunteer Force (Santa Monica, Ca.: The Rand Corporation, 1977).

For a variety of reasons, mostly economic in nature, the military enjoys an even more substantial participation in the black youth labor market than it does for the youth labor market as a whole. Unlike some areas of the civilian labor market, where blacks often face inferior economic opportunities, the military does not discriminate according to race (and this has been perceived by large numbers of black youth). As a result, blacks have historically served in larger numbers relative to their population than have nonblacks. Indeed, whereas the military employs about 10% of today's total eighteen- to twenty-four-year-old male labor force, it employs nearly 20% of the black male eighteen- to twenty-four-year-old labor force.[9] Increasing participation of black college graduates in the officer corps is perhaps even more impressive. Whereas blacks made up only 1% of all new officers in 1960, today they make up about 7% of the total.

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[9] The proportion of new recruits that are black has in fact increased significantly over the past ten to fifteen years, from about 8% in 1960 to some 16-18% today. The primary reason for this increase is the increasing proportion of black youth found eligible for military service. During the mid-1950s, only about 12% of black youth were classified into Mental Categories I-III (i.e., the upper 70% of the mental aptitude spectrum)--that is, the so-called "prime" manpower pool. Today, between 45% and 50% are so classified. As a result, blacks have increased their share of this prime manpower group from 2.9% in the mid-1950s to more than 7% today.

Thus, not only is the military an important factor in the youth labor market in total, it is of increasing importance for certain segments of that market, especially minorities and women. Stated more simply the above discussion has shown that the military is an important factor in the demand side of the youth labor force.

## Secular, Cyclical, and Seasonal Variations in the Military's Demand for Labor

Because the military plays such an important role in the youth labor market, variations in the military's demand for labor can have an important effect on both the size of the civilian youth labor force and on the employment and unemployment prospects for these youths. Three kinds of variations in the military's demand deserve attention: secular, cyclical, and seasonal.

As can be seen in Figures 1 and 2 shown earlier, there has been a secular trend toward smaller military forces since the mid-1950s (excepting of course for the Vietnam War). Looking ahead, however, we expect military forces to stay at approximately their current levels--about 2.1 million members in the active duty forces. In other words, barring major unforeseen circumstances, such as another war, we should not expect to see major secular trends in the size of military forces, and hence in the numbers of youth employed by the military.

In the case of cyclical variations, the individual military services seem to exhibit some cyclical recruiting patterns (e.g., the Navy seems to still have a four-year recruiting cycle, which is a result of the Vietnam buildup and drawdown). For the Department of Defense as a whole, however, there is not much cyclical variation in the demand for labor, simply because the cycles of the individual services tend to be offsetting. Thus, cyclical variations in the demand for labor on the part of the military would not seem to pose significant problems for the youth labor force.

Finally, there are significant seasonal variations in the military's demand for labor, but this seasonal variation is a supply-side not a demand-side phenomenon. That is, the military has adapted itself to the seasonal variations in recruits seeking to join the military. Specifically, the military recruits particularly large numbers in the summer and fall months--i.e., following June high school graduation. Again, seasonal variations in the military's demand for labor would not appear to pose significant problems for the youth labor force.[10]

In conclusion, there have been secular and cyclical fluctuations historically in the military's demand for labor, but these have been a result of declining forces sizes and the Korean and Vietnam conflicts. Barring another major buildup in force sizes, we would not expect much further secular or cyclical variation in the size of military forces, or in the military's demand for youth labor. Although there is in fact considerable seasonal variation in the military's demand for labor, this works with, rather than against, youth labor force participation, since the military is merely responding to the supply of new recruits enter the job market.

## MILITARY SERVICE AND YOUTH LABOR SUPPLY

Examination of the supply side of youth participation in the military is important for two main reasons. First, the continuity between military and civilian work experience is much greater than is sometimes perceived. That is, there are sufficient similarities between military and civilian employment that movement between the two sectors is frequently quite easy. Every year, hundreds of thousands of young men leave the civilian youth labor force to join the military, just as hundreds of thousands of young men leave the military to rejoin the civilian youth labor market. This means that modest variations in the variables affecting the desirability of military and civilian employment can have a significant effect on the flow between military and civil youth labor markets.

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[10] During the draft, there was far less seasonal variation in the demand for labor, since the military could simply draft to make up for recruiting shortfalls in the "off" recruiting months. This probably caused greater disruption to the youth labor force than the current seasonal variations which are in response to the supply of individuals entering the youth job market.

Second, military employment provides the individual with the opportunity to accumulate significant amounts of human capital. This human capital, in turn, can frequently be used to obtain subsequent civilian employment. As a result, the military can affect not only the size of the civilian youth labor force, but also qualitative aspects of that manpower pool, such as the skill and education mix of members of the youth labor force.

The following discussion examines these supply-side aspects of military employment, focusing first on the decision to join the military, then on the military work experience, and, finally, on the decision to stay or leave the military.

## The Decision to Join the Military

When an individual joins the military, he or she leaves the civilian work force. Because of this, it is important to examine various aspects of the decision to join, as discussed below.

### Factors Affecting the Enlistment Decision

Models of enlistment supply have been the subject of a considerable amount of research for at least ten years. Among those studying the enlistment decision are economists, sociologists, and psychologists. The economists tend to formulate the enlistment decision as a model of occupational choice, where the individual presumably weighs the various advantages and disadvantages of alternative employment options and chooses the one that maximizes his or her utility. Although these are certainly not the only models used to explore the enlistment decision process, they are probably better developed than those of the other disciplines.[11]

Generally speaking, models of the enlistment decision, irrespective of their disciplinary origin, have highlighted a number of factors as critical in the individual's decision about whether or not to enlist in the military. Important among these factors are certain economic variables, including the wage rate offered by the military, the potential earnings from civilian employment, and the chances for obtaining civilian employment (as reflected by the unemployment rate). Economic factors are certainly not the only variables to affect the individual's decision to enlist, as surveys consistently show a number of other factors to be as important, such as the training and job experience offered by the military, the chance to travel and "see the world," patriotism, and a host of other factors too numerous to mention.

The point is simply that the enlistment decision is shaped by many different things, so that modest changes in one or more of these can significantly affect both the number and types of individuals who join the military. To illustrate, a variety of economic supply studies conducted over the past ten to twelve years show that if the military pays a wage approximating that earned by comparably aged and educated civilian workers, the military can attract between

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[11] As various data collection techniques such as surveys have improved in recent years, so have the enlistment decision models of the other academic disciplines, especially sociology and psychology.

15% and 20% of a given youth cohort into military service.[12] Moreover, most of these studies show that the elasticity of supply with respect to pay is somewhere in the neighborhood of 1.0 (Actually, estimates of the enlistment supply elasticity range from about 0.5 to 2.0, with 1.0 representing probably the best guess.) That is, a 10% increase in the military wage rate will yield approximately a 10% increase in the supply of labor to the military. Conversely, as the chances for obtaining civilian employment decrease, the supply of labor to the military also increases. Again, most studies done over the past several years show unemployment elasticities of between 0.1 and 0.4. In other words, a 10% increase in the unemployment rate will lead to a 1% to 4% increase in enlistment supply.

The implication of these results is clear. Specifically, as the military makes its employment offer more (less) attractive relative to civilian employment opportunities, the military can expect to get more (fewer) individuals to join. In other words, there is not a well defined line separating military and civilian employment. This means that in studying the factors affecting the youth labor force and youth unemployment, it is important to recognize the role the military plays in trying to attract young men and women.

## Military Earnings

As indicated above, previous studies have shown the importance of military pay for enlistment supply. In this regard, Figure 3

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[12] See for example, Dorothy Amey, et al., Supply Estimation of Enlistees to the Military (McLean, Va.: General Research Corporation, 1976); Pre-AVF Studies with Volunteer Enlistments," in Richard V.L. Cooper, ed., Defense Manpower Policy (Santa Monica, Ca.: The Rand Corporation, forthcoming); Stuart H. Altman and Alan E. Fechter, "The Supply of Military Personnel in the Absence of a Draft," American Economic Review, vol. 57 (May 1967); and Harry J. Gilman, "The Supply of Volunteers to the Military Services," in Studies Prepared for the President's Commission on an All-Volunteer Armed Force (Washington, D.C.: Government Printing Office, 1970).

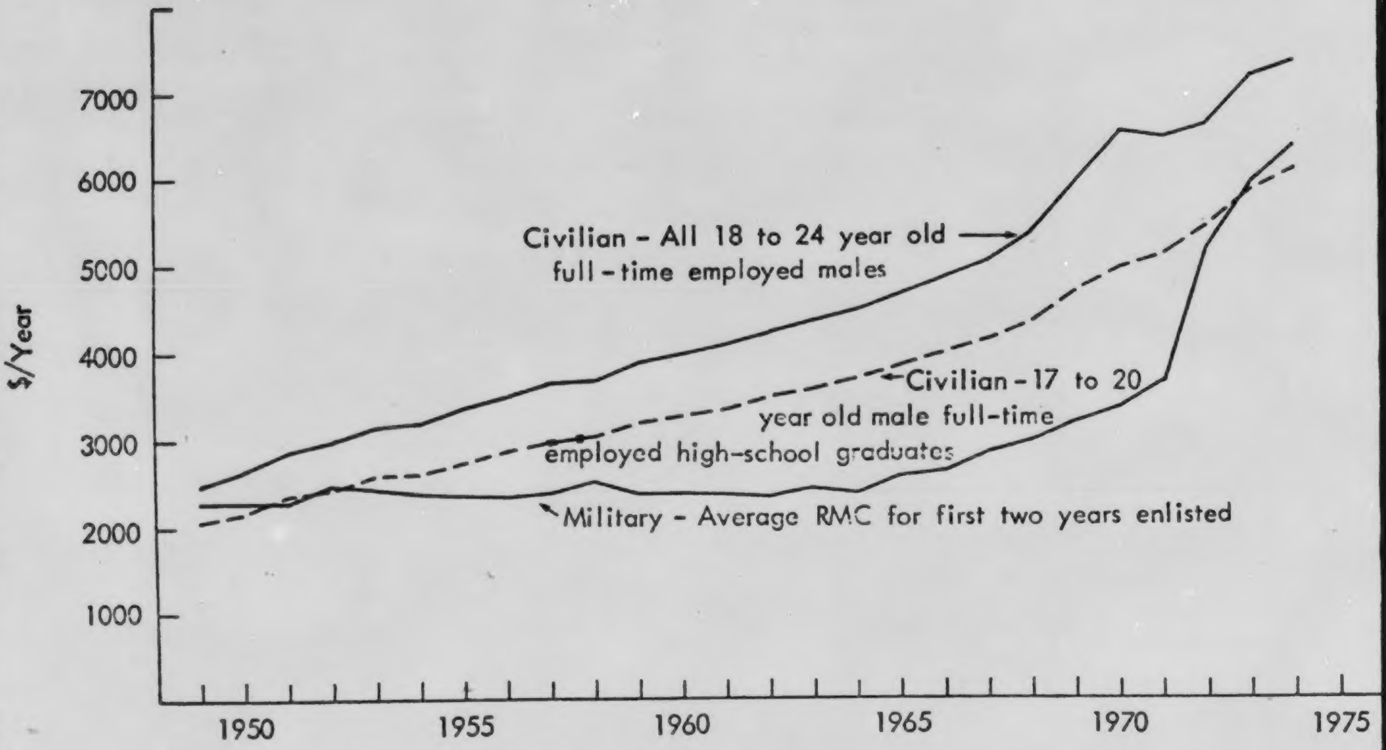

Fig. 3 — Annual Military and Civilian Wages

Source: See text.

shows that military pay for new recruits has changed significantly during the course of the past ten years. Specifically, with the pressure of the draft, there was no "need" to pay military recruits a market wage. Indeed, as shown in Figure 3, the wages earned by military recruits remained virtually unchanged from 1952 through 1965, and from 1965 to 1971 they received only cost-of-living pay increases. Although these wages were sufficient to attract some "true volunteers," the draft or threat of the draft provided the vast majority of new recruits.[13]

Given the demographic trends shown earlier in Figure 2, it is clear how inequitable the selective service draft had become by the late 1960s. That is, only about one-fifth of the male military-aged population would have to bear the burden of serving, while the other four-fifths could find more lucrative civilian employment. The President's Commission on an All-Volunteer Armed Force recommended that this pay discrimination be eliminated. Congress concurred and raised recruit pay to a level earned by comparably aged and educated civilian workers (basically seventeen- to twenty-year-old high school graduates), the results of which can be seen in Figure 3. The effect of this pay raise was substantial. In fact, by 1975 the military had doubled the number of true volunteers joining relative to ten years earlier.

Again, the more general point to be drawn from this is that as the military changes one or more aspects of its employment offer, there will be a significant impact on the size and composition of the civilian youth labor force.

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[13] During the 1950s and 1960s, about one-third of new recruits were"true volunteers"; about one-third were "draft-motivated" volunteers (i.e., individuals who volunteered in order to avoid being drafted but who would not have enlisted in the absence of a draft); and about one-third were drafted outright.

## Pre-Service Employment and the Military

It is important to recognize that many individuals joining the military come not only from the ranks of the unemployed, but directly from previous civilian employment. A recent survey by Gay, for example, shows that about 20% of the eighteen-year-old enlistees in 1974 were unemployed prior to enlisting, but that about 35% were employed part time before enlisting and 45% were employed full time.[14] For those individuals employed prior to joining the military, there were obviously aspects of the military employment offer that they found superior to their then present civilian employment. In other words, the military does often compete directly with civilian employers for young recruits. This is not altogether surprising, though, given the kinds of work that young members of the labor force often find available. That is, not only is it harder for these young members of the labor force to find work, as reflected by high youth unemployment rates, but the kinds of jobs they can obtain frequently offer less in the way of wages, challenging work, chance to accumulate human capital, and so forth, than the military can offer. Yet, it is precisely for these reasons that many young men and women seek military employment as their first or second job after leaving school.

The implication of this is clear. Although there are many young men and women who would not enter the military under any circumstances, there is a sufficiently large portion of the youth labor force that, given the proper set of inducements, would (and in fact does) join the military. Thus, analysis of the youth labor market--both supply and demand--must take the military into account.

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[14] Source: unpublished tabulations provided by Robert M. Gay, The Rand Corporation, 1976.

## The Military Work Experience

Once in the military, the individual is obligated for a period of service, generally running three to six years. That is, unlike civilian employers, the military can obligate for a period of service.[15] The discussion here looks briefly at the nature of this military work experience, including the occupations that new recruits are likely to get, as well as the accumulation of human capital by military personnel.

### Occupational Mix of the Military

Although it is most common to view the military in terms of the combat arms component, it is important to recognize that the military consists of a wide range of occupations, not at all unlike what one finds in the civilian sector. In fact, Table 3 shows that the combat arms occupations make up only about 10% of the entire enlisted work force. The other 90% is made up by such diverse occupations as aircraft mechanics, medical and dental specialists, radar repairers, radio operators, carpenters and plumbers, military police, intelligence experts, vehicle mechanics, and a host of other specific occupations. In other words, young men and women joining the military find virtually as many different types and kinds of jobs in the service as they could in the outside civilian world.

Moreover, as part of the move to the all-volunteer force, it is common to find young men and women joing the military to work specifically in a particular job or at a particular military installation, or with a particular military unit. Once frequent stories about the individual who was an engineer but forced to be an Army cook, or the cook who was forced to become a vehicle mechanic, and

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[15] There are ways, however, for the individual to get out of his or her contractual obligation.

## TABLE 3.

### DISTRIBUTION OF THE FORCE BY OCCUPATIONAL AREA: FY74[a]

| Officer | | Enlisted | |
|---|---|---|---|
| Occupation | Percent | Occupation | Percent |
| Executives | 1.6 | Combat Arms | 12.3 |
| Tactical Operations | 40.8 | Electronics | 10.4 |
| Intelligence | 3.2 | Comm/Intelligence | 6.7 |
| Engineer/Maintenance | 15.6 | Other Specialists | 1.9 |
| Scientists/Professionals | 6.6 | Elec/Mechanics | 21.6 |
| Medical/Dental | 9.4 | Medical/Dental | 4.6 |
| Administrators | 12.8 | Admin/Clerks | 18.4 |
| Supply | 6.1 | Service Supply | 11.0 |
| Other[b] | 3.8 | Craftsmen | 4.6 |
| | | Other[b] | 8.6 |

[a] Based on "primary" occupation designators.

[b] Training, Miscellaneous, and Other.

Source: Data were furnished by the Office, Assistant Secretary of Defense (Manpower and Reserve Affairs) so forth, are not only not common now, they are rare. The military services have made significant strides in better matching individuals' tastes and aptitudes with actual job assignments.[16]

The foregoing is not meant as an advertisement for military service, but rather as an indication of the very wide range of occupational specialties that young men and women joining the service can and do engage in, and as evidence that many of these young men and women are in these occupations as a result of their own choice. The implications for the civilian youth labor force are obvious, in that individuals leaving the military bring with them a set of skills acquired during their military service. To the extent that young men and women are engaged in jobs that are found in the civil sector and than these are the types of jobs in which these individuals would like to continue working, there is the important issue of the accumulation of human capital, as discussed below.

## The Accumulation of Human Capital

As indicated above, military work experience can be characterized in economic terms as the accumulated human capital. Some of this human capital will be of a very general variety, such as the maturity that goes along with the individual's early job choices. Parts of this human capital are also very specific--in fact, they are specific entirely to the military. Examples of this would be the use of mortars, marching, drill formation, and so forth. But a substantial amount of this human capital may be of the general occupational type that is transferable to similar jobs in the civil sector.

This human capital formation takes place in several ways: through formal school training, through on-the-job training (OJT), and through actual job experience. The military maintains one of the largest educational establishments in the nation. All military recruits attend basic military training--i.e., the so-called "boot camp." In addition, about 95% of all new recruits attend formal technical schools. In these schools, which last from a few weeks to as much as two years (and average three to four months in length), the individual is taught about his or her new job. The classes consist of formal lectures and training, demonstration, and actual hands-on experience.

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[16] The major problem here, however, is that young recruits are frequently unaware of what they do and do not want to do. In fact, for many it is this indecision that led to their joining the military.

The on-the-job training and actual job experience in the military constitute another form of human capital accumulation. In fact, given the often theoretical nature of formal technical school training, the OJT and job experience may constitute the more significant form of human capital accumulation.

In sum, the military work experience is likely to represent a significant amount of human capital formation by the individual. And this fact has not gone unrecognized by potential recruits, as military training and job experience are two of the most frequently cited reasons for joining in the first place.

## The Decision to Stay or Leave

Upon completing the enlistment tour, the individual must decide whether to remain in the military (if he is declared "eligible") or to return to the civilian labor force. The discussion focuses on this decision to stay or leave by outlining, first, the factors affecting the reenlistment decision: second, what individuals do in their post-service employment; and third, the migratory effects brought about by military service.

### Factors Affecting the Reenlistment Decision

As has been the case with enlistment supply, reenlistment supply has been studied extensively during the past ten years or so. Again, a number of different academic disciplines have been brought to bear on the issue, including economics, sociology, and psychology, among others. The economic studies tend to show that reenlistment supply is quite sensitive to military and civilian pay opportunities. Specifically, the elasticity of supply with respect to pay has been shown to be in the neighborhood of 1.0 to 4.0, with 2.0 probably representing the best guess.[17] This means, then, that reenlistment supply is quite sensitive to military pay. That reenlistment supply should be more sensitive to pay than enlistment supply is not surprising, though, since individuals facing the reenlistment point are those who have already entered the military. That is, the initial enlistment decision screens out those individuals most opposed to military service on "taste" grounds, so that reenlistment supply is likely to be drawing from a more homogeneous manpower pool.

For a variety of technical reasons, studies have been less successful in pinpointing the responsiveness of reenlistment to unemployment rates. Nevertheless, the conventional wisdom is that reenlistment supply is in fact quite sensitive to unemployment, and a casual review of the evidence bears this point out. Clearly, however, more thorough study of this issue is warranted before definitive conclusions can be drawn.

Noneconomic factors have also been shown to play an important role in the individual's decision about whether to reenlist. A variety of studies, mainly using survey techniques, find that certain attributes of military service will either persuade or dissuade an individual from reenlisting. For example, job security is one of the most frequently cited reasons for individuals deciding to reenlist, while the loss of personal freedom is perhaps the dominant noneconomic reason why individuals choose to leave military service after the end of their first term. Since these are clearly not the only factors that affect the individual's reenlistment decision, the larger point is simply that reenlistment is a function of many variables, such that changes in one or more of these variables can in the long run significantly affect the numbers and types of individuals reentering the civilian youth labor force.

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[17] See, for example, John H. Enns, Reenlistment Bonuses and First-Term Retention (Santa Monica, Ca.: The Rand Corporation, 1977); and Gary R. Nelson, "Economic Analysis of First-Term Reenlistments in the Army," in Studies Prepared for the President's Commission, op.cit.

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## Post-Service Employment

As indicated earlier, military service may provide the opportunity to accumulate human capital--human capital that can be applied to subsequent civilian employment. The extent to which the military does lead to the accumulation of human capital has in fact been the subject of considerable study during the past five to ten years.[18]

These studies of veterans' post-service activities have tended to focus on one or more of the following three issues: general employability of military veterans, the extent to which veterans use some of the knowledge and experience gained during the military employment for their post-service civilian employment, and the earnings of veterans as compared with nonveterans. Although these studies differ substantially in their specific findings, one general theme does emerge. Specifically, military service in general does lead to the formation of human capital that can be applied to post-service civilian employment. As would be expected, though, the degree to which human capital accumulated during military service can

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18. See, for example, Eva Norrblom, An Assessment of the Available Evidence on the Returns to Military Training (Santa Monica, Ca.: The Rand Corporation, 1977); Eva Norrblom, The Returns to Military and Civilian Training (Santa Monica, Ca.: The Rand Corporation, 1976); Robert B. Richardson, "An Examination of the Transferability of Certain Military Skills and Experience to Civilian Occupations," unpublished Ph.D. dissertation, Cornell University, 1966; William Mason, "On the Socio-economic Effects of Military Service," unpublished Ph.D. dissertation, University of Chicago, 1970; Adele P. Massell and Gary R. Nelson, The Estimation of Training Premiums for U.S. Military Personnel (Santa Monica, Ca.: The Rand Corporation, 1974); and Zvi Griliches and William Mason, "Education, Income, and Ability," Journal of Political Economy, vol. 80 (May/June 1972).

be transfered to the civilian sector is obviously greater for individuals serving in military occupations with more direct civilian counterparts.

In general, these analyses of veterans' post-service employment activities show, first, that veterans trained in skill x during the military tend to be employed in larger numbers in skill x in the civil sector than would be implied by a mere random sampling. In other words, veterans tend to gravitate towards the kinds of occupations they had in the military. Thus, military service may have a potentially important effect on shaping the kinds of occupations that members of the youth labor force will enter in their post-service careers.[19] Second, individuals employed in a post-service occupation similar to their military occupation tend to show higher earnings than their veteran counterparts whose civilian occupations are not so directly related. In other words, veterans' post-service earnings in a given occupation are higher if the veteran's military service was in a related occupational area.[20]

Part of the above-cited earnings differential may be due to a so-called "certification" effect. That is, for individuals such as minorities and high school dropouts whose economic and employment opportunities are otherwise more limited, satisfactory completion of military service may be viewed by potential employers as an indicator of greater employability. Casual examination of the evidence bears

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19. Some of the correlation between individuals' military and post-service activities is explained by the fact that individuals' pre-service and military occupations are also correlated, so to the extent that individuals' pre- and post-service occupations correlate, so will their military and post-service occupations.

20. It is important to note that the studies are far from unanimous on this point. However, the studies that fail to find much of a relation have generally been hampered by data problems. In cases where there are sufficient data, they tend to show their positive earnings effect. See, Eva Norblomm, An Assessment of the Available Evidence, op. cit.

this point out, as black and high school dropout veterans both tend to see greater gains from their military service in subsequent civilian employment than do white high school graduate veterans. In general, however, the evidence is sketchy, so further study of the effects of military service on post-service earnings and employment opportunities clearly deserves more careful attention.

Finally, military service has a significant effect on post-service employment activities through the educational and training benefits offered by the Veterans Administration. The post World War II G.I. Bill educated literally hundreds of thousands of former servicemen. Although the nature of the G.I. Bill has recently changed,[21] the military will continue to represent a potentially important source of educational benefits for America's youth population. Only recently have the effects of military service on post-service education and training begun to be studied,[22] so all the effects are far from understood.

The general conclusion to emerge from this review of post-service activities is that military service may have important effects not only on the size of the youth labor force, but perhaps more important, on the composition of that labor force--especially the education and skills possessed by former service personnel reentering civilian life.[23]

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[21] The present day GI Bill is contributory. That is, for every dollar that the service member contributes to his or her post-service education "fund", the Government contributes two dollars.

[22] See, for example, Dave M. O'Neill, Sue Goetz Ross, and John T. Warner, "The Effect of Military Training and GI Bill Training on Civilian Earnings," in Richard V.L. Cooper, ed., Defense Manpower Policy, op. cit.

[23] Note, however, that this does not imply that the current military-civilian mix of education, training, and experience is efficient. Rather, it merely indicates that there may be some important effects. The relevant policy questions--e.g., what is the "right" amount of military education and training from a social perspective?--obviously need to be examined in a broader context.

At the same time, definitive conclusions about the magnitude of these effects (or, in some cases, even their direction) must be tempered by the host of sample problems that the analyst encounters with most data sources. For example, studies of post-service earnings encounter a number of selectivity biases regarding who serves in the military and who does not, and who leaves the military and who does not.

More definitive conclusions regarding these effects must therefore await improved data and analysis. Indeed, one of this paper's principal recommendations is that special labor force data collection efforts (e.g., the National Longitudinal Survey) specifically focus on collecting information about all civilian and military work experience.

## Migration

Another possible effect of military service is on migration patterns of American's youth. These effects have been relatively unstudied thus far, but we do know that military retirees, for example, tend to locate in disproportionately large numbers near military installations. This is because of the benefits provided near military installations (e.g., commissaries, free medical care, and so forth). Military service may have a similar effect on one-term veterans in terms of geographic migration and location, and these effects clearly need to be studied further.

## THE MILITARY AND YOUTH LABOR STATISTICS

Two important conclusions emerge from the preceding discussion. First, the military's demand for labor is an important determinant of both the size and composition of the youth labor force. This means that changes in the military's demand for labor can have significant effects on the youth labor market, including employment prospects, the size of the youth labor force, and a host of other variables affecting American youth. Second, the military also exerts a major influence on the supply-side behavior of the youth labor force. Specifically, American youth have demonstrated a considerable degree of mobility between the military and civil sectors. This includes both the initial decision about joining the military and later decisions about whether to remain in or leave the military. Perhaps the most significant from the point of view of the civilian labor market is the human capital that former Service members bring back with them when they rejoin the civilian work force.

The implications of these findings for the measurement and collection of youth labor market information are several fold. Specifically, these implications concern (1) the measurement of youth unemployment rates, (2) the collection of aggregate labor force statistics, and (3) the collection of special labor force data depending upon the particular policy purposes for which this information is to be used. In this regard, labor force statistics would seem to have at least two important policy uses: (1) as an indicator for monitoring the "health" of the economy targeting particular government programs, and (2) as a research tool for investigating the determinants of labor supply and demand. The discussion below briefly examines the appropriateness of including military personnel in labor statistics from the perspective of each of these policy purposes.

In the first instance, labor force statistics--especially unemployment rates--provide useful measures for assessing the economy's "health." The results presented in this paper suggest that, for both theoretical and empirical reasons, military personnel ought to be viewed as being part of the labor force. On theoretical grounds, military service is little different from many other employers. Military service is entirely voluntary,[24] so the military competes as just one among many employers of the nation's youth. This is borne out empirically as well. Not only is military service apparently seen as an attractive employment option by a significant portion of the youth labor force--between 10 and 20 percent of young men enter the military--but members of the youth labor force have in fact demonstrated considerable mobility between the military and civil labor markets.[25] In other words, military service is an integral part of the employment pattern for a sizeable portion of the youth labor force.

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[24] On theoretical grounds, a case can be made (though perhaps not too persuasively) for excluding the military during periods of the draft. That is, since not all military participation is voluntary during a draft, military may be a more separate and distinct entity from civilian employment when a draft is present.

In terms of measuring unemployment rates, the above discussion suggests that those employed in the military ought to be counted as part of the labor force. Indeed, there is no more reason for excluding military personnel from labor statistics than there is for excluding other government employees, or any other employees for that matter. Because those serving in the military are employed, historical measurements based on the civilian labor force alone have therefore overestimated "true" unemployment rates, and in some cases substantially.[26] For example, the current procedure overestimates overall unemployment rates among black youth by some 15 to 25 percent--as opposed to a 10 percent or so overestimation of white youth unemployment rates--simply because such a large fraction of the black youth labor force is employed in the military.[27] Although this does not dismiss the problem of black unemployment, it does suggest that the problem is not quite as severe as a casual

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25. If, on the other hand, the military was empirically a distinct entity from the civilian sector, as in fact has been the case in some countries, then a case could be made for excluding the military from labor force measures on empirical grounds. That is, if there is little flow between the military and civil employment markets, then the inclusion or exclusion of the military from labor force measures is both less interesting and less important.

26. That is, the current procedure omits those employed in the military from the denominator of the ratio used to calculate the unemployment rate.

27. Moreover, because the military tends to select the "cream of the crop" from among black youth, those not serving in the military would be expected to be the ones that would have the most difficulty securing employment, from either military or civilian employers.

review of unemployment rates for black youth in the civilian labor force would seem to imply. More generally, when the purpose is one of assessing how well the economy is doing with respect to employing the members of its labor force, the foregoing suggests that military personnel ought to be included in labor force statistics.

At the same time, the foregoing should not be viewed to mean much change in the ways that data are collected or maintained. That is, although it seems desirable to define unemployment rates relative to the total labor force, it is useful to maintain separate statistics on the civilian labor force and on the total labor force, as is now the case. This is simply an example of a more general principle. Specifically, labor force statistics ought to be maintained for major segments of the youth labor market--e.g., according to age, race, geographic location, and so forth. That is, collecting and maintaining labor statistics according to these major segments of the youth labor force helps to pinpoint and spot certain problem areas. Because the military is in fact an important segment of the youth labor force, this suggests that the current procedure of maintaining separate labor force statistics for military and civilian youth employment is a valid one.

Besides their uses in monitoring aggregate employment and unemployment activity, labor statistics provide the core tool for investigating the determinants of labor force activity: participation, employment, earnings, occupational choice, and so forth. In this regard, this paper has argued vigorously that the military represents a potentially important source of human capital for a sizeable portion of American youth. The limited knowledge currently available suggests that military service can have significant effects on individuals' occupational choices, migration, and earnings, among other things.

Since the collection of individual data, both cross sectional (e.g., the CPS) and longitudiual (e.g., the NLS), represents the primary source for investigating the determinants of labor force activity, the foregoing argues strongly for incorporating information about individuals' military experience into these data collection efforts. For example, data collection efforts such as the National Longitudinal Survey, the Current Population Survey, the Census, and so forth, should place greater emphasis on collecting relevant information about the individual's military participation. This would include age of entry into the military, length of service, rank, occupational specialty, training received (both formal and on-the-job), and so forth. To the extent that current data sources do not properly measure this training and experience, then we are failing to recognize the full effects of alternative policy options. Therefore, greater attention needs to be directed toward collecting relevant information about the individual's military experience, in conjunction with his or her civilian experience.

In general, this calls for greater coordination of data collection efforts between military and civilian authorities. Such efforts are warranted, not only because youth labor markets are affected by both military and civilian work experience, but also because the controlled nature of the military environment affords the opportunity to examine certain youth productivity and behavioral parameters that can be observed only with great difficulty in the civilian sector. For example, recent surveys of military personnel have provided valuable information about the relationships among individuals' attitudes, their earnings prospects, and their actual employment decisions.[28] This was made possible largely by the fact that the military was able to first survey these individuals, then track and compare these individuals' actual decisions with what they said they had intended. Combined with the vast amounts of information available on the military personnel files, these types of surveys can be used to provide valuable information about individuals' attitudes, supply behavior, and productivity--not only in the military, but in the civilian labor market as well.

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[28] A number of recent data collection efforts illustrate the point. For example, to estimate the response to the military's health professionals scholarship program, a survey was administered to a stratified random sample of medical students across the country. The survey essentially asked these students to trace out their supply curve, not only with respect to values of the stipend, but also to such other variables as tour commitment, residency policy, and so forth. The results have been impressive, as prediction errors over the past three years have been in the neighborhood of 5 percent. In another effort, individuals' productivity on the job was measured. Combined with the attitudinal information also collected and the information available from personnel files, valuable insights have been gained about the determinants of productivity, attitudes, and supply behavior.

Areas where such cooperation and coordination between military and civilian data collection efforts could prove to be particularly profitable include certain ongoing and special civilian collection efforts, including future National Longitudinal Surveys, the Current Population Survey, and the Census. Alternatively, certain regular and special data collection efforts in the military could provide valuable information to civilian researchers and policy makers. Examples include the annual survey of new recruits at armed forces entrance and examination stations, exit and post-service surveys conducted by the military, attitudes of young women toward military service, and so forth.

In sum, this paper has argued that the military is a most important factor in youth labor markets. On the one hand, the military is a major source of jobs for American youth, while on the other, it is clear that military experience and training can play a significant role in the issues and problems of civilian labor market behavior. It is time that labor force statistics reflected this important role of the military.

# DIRECT EFFECTS OF EMPLOYMENT AND TRAINING PROGRAMS ON EMPLOYMENT AND UNEMPLOYMENT: NEW ESTIMATES AND IMPLICATIONS FOR EMPLOYMENT POLICY

By: Charles C. Killingsworth and Mark R. Killingsworth

## ABSTRACT

We first discuss the ways in which participants in employment and training programs are counted in official figures on the number of persons employed, unemployed and not in the labor force. We then analyze the consequences of an expansion of such programs, and present estimates of the purely"statistical" effects of expanding such programs on the number of persons employed, unemployed and not in the labor force. These effects appear to be substantial. For example, our estimates imply that, in the absence of any employment and training programs, the aggregate unemployment rate in 1976 would have been 0.8 percentage points higher than the actual figure, and the teenage unemployment rate would have been 3.9 percentage points higher than the actual figure. We conclude by discussing the implications of our analysis and estimates for employment policy and employment training programs as an instrument of employment policy.

## INTRODUCTION

How do employment and training programs affect program participants, and what is the appropriate role for such programs in employment policy?* Most research on the effects of employment and training programs, and most discussions of their merits as an instrument of employment policy, focus on what may be called "ultimate" or "long run" effects: on the extent to which they affect the human capital (and thus the subsequent earnings and employment) of enrollees, and on the extent to which they change the structure of the labor force, shifting the economy's Phillips curve and reducing the unemployment rate associated with any given inflation rate. But a few studies (Cohen, 1969;

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Small, 1972) suggest that employment and training programs also have "short run" or purely "statistical" effects on employment and the unemployment rate: depending on the particular program in which participants are enrolled, enumerators for the Current Population Survey (CPS) count program participants as employed, unemployed or not in the labor force. Obviously, then, regardless of the ultimate impact of these programs on enrollees' earnings and employment, changes in program enrollments necessarily have a direct or short run or statistical impact on official CPS figures on employment and on the unemployment rate.

In this paper we have two objectives. First, relatively little is known about the magnitude of the statistical effect of employment and training programs, so our "short run" or "statistical" objective is to present estimates of the magnitude of this effect, with special reference to youth. We also have a "long run" or "analytical" objective, however: to discuss the role of employment and training programs as an instrument of employment policy. For it turns out that our analysis of the "statistical" effects of employment and training programs not only provides estimates of these effects but also has a number of important implications for broader issues of employment policy.

## THE TREATMENT OF EMPLOYMENT AND TRAINING ENROLLMENT IN OFFICIAL LABOR FORCE STATISTICS

The substantial growth in enrollment in employment and training programs during the 1960s--from 50,000 to nearly 1,000,000 in 1971--posed a serious question for Federal statistics-takers: how should such enrollees be classified in official labor force statistics? In 1965, a Federal interagency committee adopted definitions which have been followed since that time. As Small (1972, p. 7) puts it:

> In the final analysis, persons (enrolled in such programs) were to be classified as employed if they were receiving wages rather than subsistence or other allowances, or if they were getting on-the-job training; as unemployed if they were enrolled in institutional (classroom) training.

> Job Corps participants were to be counted as not in the labor force. The household interviewers (for the CPS) were given detailed instructions for classifying those who volunteered information on participation.
>
> Most of the large manpower programs involved placing people in jobs rather than in classroom training, and most participants would therefore be counted among the employed. This would probably be true whether or not the household respondent offered any information on the enrollee's specific program participation, because holding a job and receiving a wage are fairly clearcut unambiguous activities. On the other hand, the line of demarcation between being unemployed and outside the labor force is much harder to define. Thus, how participants in programs such as the Work Incentive program are actually classified in the official labor force survey is uncertain.

Accordingly, employment and training programs may be classified according to the way in which their enrollees are counted in official employment and labor force statistics. We shall refer to programs whose enrollees are counted as not in the labor force--e.g., the Job Corps--as "Type I" programs; we shall refer to programs whose enrollees are counted as unemployed--e.g., institutional training programs--as "Type II" programs; and, finally, we shall refer to programs whose enrollees are counted as employed--e.g., Public Service Employment or on-the-job training programs--as "Type III" programs.

Table 1 shows how enrollees in each of the major employment and training programs undertaken by the Federal government during 1963-76 were classified in CPS unemployment and labor force statistics.[1] Table 2 summarizes total average annual enrollments in each type of program both for the general population and for youth (persons age

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[1] For a capsule summary of the history and nature of many of these programs, see U.S. Department of Labor (1970, pp. 193-197).

TABLE 1.

MAJOR FEDERAL EMPLOYMENT AND TRAINING PROGRAMS 1963-76,
BY LABOR FORCE CLASSIFICATION OF ENROLLEES

| Program | Remarks |
|---|---|
| Type I programs (enrollees counted as not in the labor force) | |
| Job Corps | began 1965; continued under CETA |
| Type II programs (enrollees counted as unemployed) | |
| Manpower Development and Training Act (MDTA) - institutional training | began 1962; subsumed under CETA |
| Work Experience Program - programs other than on-the-job training | began 1965; subsumed under WIN |
| Work Incentive Program (WIN) - programs other than on-the-job training | began 1969 |
| Comprehensive Employment and Training Act (CETA) - institutional training under Titles I, II, VI | began 1974 |
| Type III programs (enrollees counted as employed) | |
| Manpower Development and Training Act (MDTA) - on-the-job training | began 1962; subsumed under JOP |
| Work Experience Program - on-the-job training | began 1965; subsumed under WIN |
| Work Incentive Program (WIN) - on-the-job training | began 1969 |
| Neighborhood Youth Corps (NYC) - in-school and summer; out of school | began 1965; subsumed under CETA |
| College Work Study | began 1965 |
| Comprehensive Employment Program (CEP) | began 1967; subsumed under CETA |
| Operation Mainstream | began 1965; subsumed under CETA |
| Public Service Careers (PSC), including New Careers and STEP | began 1970 (New Careers began 1967); subsumed under CETA |
| Job Opportunities in the Business Sector (JOBS) - Federally-subsidized | began 1968; subsumed under CETA |
| Jobs-Optional (JOP) | began 1971; subsumed under CETA |
| Public Employment Program (PEP) | began 1971; subsumed under CETA |
| Comprehensive Employment and Training Act (CETA) - on-the-job training, public service employment (PSE), adult work experience, youth employment programs | began 1974 |

sixteen to nineteen).[2] Depending on the particular kind of activity in which they were engaged (e.g., on-the-job training or institutional training), enrollees in the Work Incentive Program (WIN) were counted either as employed or unemployed; hence, WIN is in effect a blend of Types I and II and so WIN enrollments are shown separately in Table 2.

These tables reveal that the great majority of enrollees were enrolled in "Type III" programs--that is, were counted as employed during the period of their enrollment. In recent years, aggregate enrollment in all programs combined has been high both in absolute terms (e.g., in excess of 1,000,000) and relative to the level prevailing in the 1960s.

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[2] With some exceptions, the entries in Table 2 represent actual annual averages of current enrollment in each kind of program. These were calculated by aggregating annual average enrollment figures, taken from a variety of sources (e.g., issues of the Employment and Training Report of the President), for each of the individual programs listed in Table 1. Two major exceptions are as follows. First, available data for enrollments under CETA during 1974-1976 provide information one new (rather than total current) enrollments by quarter; for this reason, the entries in Table 2 for 1974-1976 include annual averages of new (rather than total current) CETA enrollments. To the extent that enrollees in CETA programs remained enrolled for more than a quarter, the 1974-1976 figures shown in Table 2 of course understate the actual total current annual enrollments. Second, available data for enrollments under the College Work Study program during 1965-1976 refer to awards (in effect, "slots") available during a given fiscal year rather than to actual recipients (enrollees on work-study payrolls) during a given calendar year. In arriving at the figures shown in Table 2, we treated fiscal and calendar year figures as equivalent, and multiplied the number of awards by 0.8 to obtain an estimate of the annual average number of actual recipients. Further, between fiscal (or, under our assumptions, calendar) years 1975 and 1976, the number of awards increased sharply, from 570,000 to 973,000--so sharply that we suspect that colleges and universities did not adjust in time to fill as much as 80 per cent of the maximum number of slots available in 1976. In arriving at the figures shown in Table 2, we therefore assumed that the "effective" number of awards available in 1976 was midway between the 1976 figure and the figure for 1977, 895,000, and multiplied this "effective" number by 0.8 to get an estimated annual average number of actual recipients during 1976.

## TABLE 2.

## TOTAL ENROLLMENT AND YOUTH ENROLLMENT IN MAJOR FEDERAL ENPLOYMENT AND TRAINING PORGRAMS 1963-76, BY TYPE OF PROGRAM

(Annual Averages, In Thousands Of Persons)

| | WIN/Work Experience | | Type I | | Type II | | Type III | | Total | |
|---|---|---|---|---|---|---|---|---|---|---|
| Year | Total | age 16-19 | Total | age 16-19 | Total | age 16-19 | Total | age 16-19 | Total | age 16-19 |
| 1963 | 0 | 0 | 0 | 0 | 22 | 3 | 2 | * | 24 | 3 |
| 1964 | 0 | 0 | 0 | 0 | 43 | 8 | 10 | 1 | 53 | 10 |
| 1965 | * | * | 10 | 9 | 63 | 17 | 170 | 126 | 243 | 152 |
| 1966 | * | * | 28 | 26 | 62 | 14 | 395 | 297 | 485 | 337 |
| 1967 | * | * | 42 | 38 | 48 | 12 | 542 | 401 | 632 | 450 |
| 1968 | 3 | * | 33 | 30 | 53 | 13 | 543 | 399 | 632 | 442 |
| 1969 | 59 | 6 | 24 | 21 | 48 | 10 | 624 | 421 | 755 | 458 |
| 1970 | 91 | 10 | 20 | 15 | 51 | 9 | 674 | 462 | 836 | 497 |
| 1971 | 109 | 12 | 21 | 16 | 58 | 13 | 756 | 503 | 943 | 543 |
| 1972 | 115 | 14 | 22 | 17 | 53 | 10 | 1001 | 600 | 1191 | 641 |
| 1973 | 163 | 18 | 20 | 15 | 38 | 8 | 893 | 529 | 1114 | 570 |
| 1974 | 212 | 21 | 19 | 15 | 86 | 24 | 884 | 494 | 1202 | 555 |
| 1975 | 215 | 24 | 20 | 16 | 133 | 49 | 1264 | 655 | 1632 | 743 |
| 1976 | 265 | 29 | 20 | 16 | 159 | 46 | 1420 | 748 | 1864 | 839 |

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Notes: * = negligible. Due to rounding, column entries do not always sum along rows to "Total" figure shown in last column. For sources, see text.

But while the size of these enrollments is substantial, the existence of substantial employment programs sponsored by the federal government is not, of course, unprecedented: enrollments in federal work-relief programs during the 1930s were still larger. There is, however, an interesting difference: in contrast with the procedures adopted in 1965, under which Type III enrollments of the 1960s and 1970s are counted as employed, participants in these work-relief programs during the 1930s were--and, in the historical statistics, still are--counted as unemployed. (For elaboration on this point see Darby, 1976, pp. 2-8, and the references therein.) Table 3 illustrates the consequences of counting 1930s work-relief program participants as employed (as they would be counted under current procedures) rather than as unemployed (as they were, and still are, counted in official statistics).[3] Presumably, the growth in employment and training program enrollments during the 1960s and 1970s may also have had an important effect

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[3] Darby gets "recalculated" figures (or, in his terminology, "corrected" figures) which differ somewhat from ours. (Compare, for example, the fourth column in Table 3 in Darby, 1976, p. 8, with the third column in our Table 3.) These differences seem to be the result of using different data on work-relief enrollment: Darby uses data on total Federal, state and local work-relief enrollment appearing in a 1966 Department of Commerce publication (see notes to Table 2 in Darby, 1976, p. 7, especially in reference to column 7 of that table); while we use data on Federal work-relief enrollment only appearing in the 1943 and 1946 editions of the Statistical Abstract of the United States (see notes to our Table 3). Despite these differences, our "recalculated" unemployment rate figures for 1933-1941 are similar to Darby's "corrected" figures for the same years; and, like us, Darby concludes that classifying work-relief enrollees as employed rather than unemployed would have a substantial effect on measured unemployment rates for those years.

## TABLE 3.

## PARTICIPANTS IN FEDERAL WORK-RELIEF PROGRAMS AND REPORTED AND RECALCULATED UNEMPLOYMENT RATES, 1933-42

(Annual Averages)

| Year | Work-Relief Program Participants* (thousands) | Unemployment Rates | | Percentage reduction in unemployment rate**** |
|---|---|---|---|---|
| | | Reported** | Recalculated*** | |
| 1933 | 4151 | 24.9 | 18.3 | 26.5 |
| 1934 | 661 | 21.7 | 20.7 | 4.6 |
| 1935 | 3817 | 20.1 | 13.8 | 31.3 |
| 1936 | 3666 | 16.9 | 10.8 | 34.9 |
| 1937 | 2553 | 14.3 | 10.0 | 30.1 |
| 1938 | 4210 | 19.0 | 12.3 | 35.3 |
| 1939 | 3246 | 17.2 | 12.0 | 30.2 |
| 1940 | 2869 | 14.6 | 10.0 | 31.5 |
| 1941 | 1767 | 9.9 | 7.0 | 29.3 |
| 1942 | 386 | 4.7 | 4.1 | 12.8 |

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Notes:

* = includes Civil Works Administration, Civilian Conservation Corps, National Youth Administration, Works Progress Administration and other Federal agency projects financed from emergency funds. Data do not include administrative personnel. (Source: Statistical Abstract of the United States, 1943 edition, Table 204; and 1946 edition, Table 259.)

** = Source: 1947 Handbook of Labor Statistics, Table A12.

*** = Calculated by switching all participants in Federal work-relief programs from "unemployed" to "employed" category.

**** = (column 2 - column 3)/column 2.

on reported employment and on the reported unemployment rate.[4] But how may one measure the purely "statistical" effects of these programs, and just how large has this effect actually been?

## MEASURING THE STATISTICAL EFFECTS OF EMPLOYMENT AND TRAINING PROGRAMS

For purposes of measuring the purely statistical effects of an expansion of employment and training programs, it is useful to divide the total effect of such expansion into two components: "immediate" and "induced." First consider the immediate effect of each kind of program. When a person enters a Type I program, his post-enrollment status is always "not in the labor force," so that the immediate effect of an expansion of a Type I program is to increase the number of persons counted as absent from the labor force by an amount equal to the number of enrollees who were in the labor force (that is, were employed or unemployed) prior to enrollment; and to reduce the numbers

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4. Note that the third column in our Table 3 does not show what unemployment rates would have been during 1933-1941 in the absence of government work-relief programs. It simply shows what official unemployment rate statistics for these years would look like if the official definition of "unemployment" used for those years were the same (with respect to enrollees in work-relief and similar government programs) as the one in current use. Hence a comparison of the second and third columns of Table 3 reveals only "the impact of this purely definitional change"; in and of itself, it does not have any particular analytical importance, since it does not show the difference, under either definition, between reported unemployment rates and the unemployment rates which would have prevailed in the absence of work-relief programs. Though he never says so in so many words, Darby seems to believe that the "corrected" (in his terms") or "recalculated" (in our terms) unemployment rate would have prevailed either in the presence or the absence of work-relief programs. In particular, he appears to believe that such programs "crowded out" other employment on a one-for-one basis or, more or less equivalently, that the statistical effect of such programs on recorded aggregate employment (under either the 1930s or the current definition) was zero. (See Darby, 1976, pp. 5-6, especially the second complete sentence on p. 6.) Our analysis in Section II and in Appendix A implicitly calls this notion into question. For additional criticism of Darby's "crowding-out" assumption, criticism which is addressed explicitly to Darby's paper, see Kesselman and Savin (1977).

of persons counted as employed or unemployed by amounts equal to the numbers of enrollees who were employed or unemployed, respectively, prior to enrollment. (Of course, persons not in the labor force prior to enrollment are still counted as not in the labor force after they enter a Type I program.) Similarly, on net the immediate effect of expanding a Type II program is to increase the number of persons counted as unemployed by an amount equal to the number of enrollees who were not unemployed (that is, were either employed or not in the labor force) prior to enrollment; and to reduce the numbers of persons counted as employed or not in the labor force by amounts equal to the numbers of enrollees who were employed or not in the labor force, respectively, prior to enrollment. (Of course, persons who were unemployed prior to enrollment are still counted as unemployed after they enter a Type II program). Finally, on net the immediate effect of expanding a Type III program is to increase the number of persons counted as employed by an amount equal to the number of persons who were not employed (that is, were either unemployed or not in the labor force) before they entered the program; and to reduce the numbers of persons counted as unemployed or not in the labor force by amounts equal to, respectively, the numbers of enrollees who were unemployed or not in the labor force prior to enrollment. (Of course, persons who were employed prior to enrollment are still counted as employed after entering a Type III program.)

Hence it is quite simple and straightforward to work out the "immediate" effect on reported labor force statistics of expansion of any of the employment and training programs just mentioned. Describing the "induced" effects on reported labor force statistics is more difficult. This is because these "induced" effects refer (i) to the reaction of the rest of the economy to enrollment increases or decreases in the "employment and training sector" and (ii) to the reaction of the rest of the economy to any changes in the economic environment which accompany and are the consequence of such enrollment changes. It is usually possible to say what the direction of these "induced" effects on the rest of the economy will be. (For example, it seems reasonable to suppose that employers will fill at least some of the jobs which are vacated by employed persons who quit in order to enter employment and training programs.) However, the magnitude of such effects is difficult to determine _a priori_ without certain kinds of quantitative information(e.g., on elasticities of demand, supply and substitution); and in general this information is neither readily available nor, even if available, very reliable.

For this reason, previous work on the question of the statistical effects of employment and training programs (see Cohen, 1969, and Small, 1972) has been based on an _ad hoc_ assumption of convenience about aggregate labor market behavior: that measures of what we have here called the "immediate" effects of the expansion of such programs on reported aggregate labor force statistics provide a satisfactory approximation to the _total_ effects of such programs on aggregate labor force statistics; or, more or less equivalently, that "induced" effects of such programs on these statistics are of secondary importance and may be neglected in arriving at an approximate measure of the total statistical effects of employment and training programs.

Hence, for example, this amounts to an assumption that increasing Type I program enrollment reduces reported employment by an amount equal to the number of new enrollees who were in the labor force prior to enrollment. The assumption may also be applied in reverse: here it amounts to an assumption that abolishing a Type I program would increase reported employment by an amount equal to the number of enrollees who were employed prior to enrollment, and would increase the reported labor force by an amount equal to the number of enrollees who were in the labor force prior to enrollment. Similarly, in the case of Type II programs, the _ad hoc_ assumption of convenience amounts to an assumption that abolishing a Type II program would increase reported employment by an amount equal to the number of enrollees who were employed prior to enrollment, and would reduce the reported labor force by an amount equal to the number of enrollees who were not in the labor force prior to enrollment. Finally, in the case of Type III programs, this amounts to an assumption that abolishing a Type III program would reduce reported employment by an amount equal to the number of enrollees who were not employed prior to enrollment, and would reduce the reported labor force by an amount equal to the number of enrollees who were not in the labor force prior to enrollment.

It is important to note that this ad hoc assumption of convenience refers only to aggregate totals for persons employed, unemployed and not in the labor force, and does not entail any assumptions about the effect of abolishing (or increasing enrollment in) any program on the labor force status of any particular individual, enrolled or not. For example, the assumption does say that, if all of the enrollees in a Type I program were in the labor force prior to enrollment, then abolishing that program would increase the reported labor force by an amount equal to the number of enrollees. But the assumption does not imply anything about the effect of abolishing the program on the employment status of any particular individual whether previously enrolled in the program or not. In fact, if one is interested only in measuring the effects of expanding or contracting employment and training programs on aggregate labor force statistics, then, in general, assumptions or results about the effects of such changes on the labor force status of given individuals are neither necessary nor particularly relevant. All that is required is the ad hoc assumption of convenience which we have stated above.[5]

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5. In fact, neither Small nor Cohen states the assumption in the form given here. Rather, they both say that, in deriving estimates of the total statistical effects of employment and training programs, they assume that "enrollees would have continued at their previous (i.e., pre-enrollment) employment status during their participation in the federal manpower program if there had been no federal manpower program" (Cohen, 1969, p. 498), or, equivalently, that "enrollees would have continued their pre-enrollment status in the absence of the programs" (Small, 1972, p. 11). This seems to have given rise to a fair amount of unnecessary confusion and doubt as

Since it refers to aggregates rather than to particular individuals, the *ad hoc* assumption of convenience used in previous work on the statistical effects of employment and training programs is not unlike the assumptions used to construct and interpret supply and demand schedules. For example, the point of intersection of supply and demand for labor schedules gives the total number of persons who will be employed in equilibrium at a given point in time, but it does not show which persons will be employed. To say anything about the latter will require aditional assumptions (e.g., about labor turnover); but such assumptions are unnecessary if the objective is simply to discuss the aggregate level of employment. Indeed, this similarity between the *ad hoc* assumption of convenience about the effects of employment and training programs on aggregate labor force statistics and the assumptions used to construct and interpret supply and demand schedules is an advantage. As will become evident below and in Appendix A, this similarity makes it possible to analyze the effects of such programs on aggregate labor force statistics within the context of a simple model of supply and demand in the labor market.

In sum, the *ad hoc* assumption of convenience discussed above constitutes an *ad hoc* estimation method--or AHEM, for short--which may be used to estimate the effects of employment and training programs on aggregate statistics on persons employed, unemployed and not in the labor force. AHEM estimates refer to what we have called

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5. (cont.) to the validity of their estimates, since this version of the assumption is extremely stringent and, as Smith (1971, 1973) has noted, is probably quite unrealistic. However, it is important to note that Cohen and Small discuss only the effects of employment and training programs on *aggregate* labor force statistics, so that none of their work actually requires this extremely strong assumption about effects on particular *individuals*. The only assumption required for any of their estimates is the much more general *ad hoc* assumption of convenience, stated in the text, about *aggregate* behavior.

"immediate" program effects only, and ignore all "induced" effects of the expansion of these programs on the rest of the economy. Intuition therefore suggests that AHEM estimates of the total statistical effect of such programs are probably conservative. In particular, it can be shown that, because of the nature of the "induced" effects neglected by the AHEM procedure, the AHEM technique is always downward-biased: AHEM estimates will consistently overstate any decreases in employment, labor force or the unemployment rate which are attributable to increases in program enrollment and will consistently understate any increases in these variables which are the result of increases in enrollments.

In Appendix A we present a technical discussion of the reasons why this is so. Appendix A analyzes the statistical effects of employment and training programs in the context of a formal (though relatively simple) model of the labor market, and as such may be of some interest to the technically-oriented reader (e.g., the professional economist). To summarize the results in Appendix A for the general reader, we think it sufficient, for our present purposes, to note that Appendix A suggests that there are two basic reasons why the AHEM procedure is "conservative" in the sense given above. First, the AHEM procedure ignores the fact that some of the vacancies created in the rest of the economy when employed workers depart to enroll in employment and training programs will be filled with new employees, so that, even when the labor market clears, employment in the rest of the economy is not reduced on net by as much as the number of new enrollees who were employed in the rest of the economy prior to enrollment. Second, to the extent that markets do not clear--or, loosely speaking, to the extent that the economy is below "full employment"--and the government decides to expand aggregate demand by expanding employment and training program enrollment, the AHEM procedure also ignores the multiplier effects on employment in the rest of the economy which occur as the result of this expansion in aggregate demand.

With this as prologue, it is not hard to discern the general nature of the statistical effects of employment and training programs in the United States in the 1960s and 1970s. In essence, Type III programs take persons out of the "unemployed" and "not in the labor force" categories and put them into the "employed" category; and these "immediate" effects of Type III programs are reinforced by additional "induced" effects: employers fill some of the jobs vacated by the employed persons who enroll, and the additional aggregate demand generated by the expansion of the programs stimulates aggregate employment still further. Moreover, Type III program enrollments have generally been much larger than enrollments in other programs (see Table 2), so, loosely speaking, "As Type III programs go, so goes the economy." This means that, on the whole, the expansion of enrollment in employment and training programs which occurred during the 1960s and 1970s increased both recorded employment and the recorded labor force to levels above (and reduced the recorded unemployment rate to a level below) what would have prevailed in the absence of these programs. But what is the magnitude of these effects, and what implications do they have for employment policy? We turn to these questions in the next two parts of the paper.

## ESTIMATES OF THE STATISTICAL EFFECTS OF EMPLOYMENT AND TRAINING PROGRAMS

In principle, one could estimate the direct effects of employment and training programs using either of two procedures. First, one could specify a complete model of labor markets, representing both the supply of and demand for enrollments in employment and training programs explicitly in that model; estimate the parameters of the model; and then measure the direct effects of employment and training programs by (for example) simulating the behavior of the model with the supply of enrollments set at zero and then comparing these results with observed behavior. Unfortunately, specifying and then estimating such a model for use in simulations of this kind is no simple task; doing the job properly may not require building a complete macromodel, but it certainly calls for more than a few ordinary least squares regressions--and it probably calls for more data than are actually available. Developing estimates based on a complete model of labor markets is an important goal for future research; for the present, we shall content ourselves with presenting estimates of the direct effects of employment and training programs derived from the second procedure, AHEM. In view of the discussion above, it is worth stressing once again that estimates based on AHEM are conservative: if anything, the "true" statistical effects are probably greater than the estimates which we shall present below.

With this in mind, consider Tables 4-7, in which we present official CPS figures on aggregate unemployment, employment, etc., and AHEM estimates of what these labor market variables would have been in the absence of any employment and training programs. (For a detailed discussion of the procedures used in deriving the AHEM estimates set out in Tables 4-7, see Appendix B.) Table 4 contrasts CPS figures on actual levels of aggregate employment, unemployment and labor force (that is, the actual values of these labor market variables, observed in the presence of employment and training programs) with AHEM figures (that is, our estimates of what these variables would have been in the absence of the programs) for the total population. Hence the columns headed "diff" in Table 4 gives the purely statistical effect of employment and training programs on the variables considered there. Table 5 presents similar comparisons for the aggregate employment-population ratio, the unemployment rate and the labor force participation rate for the population as a whole. Finally, Tables 6 and 7 present analogous results for the teenage population (persons age sixteen to nineteen).

Both for the population as a whole and for the teenage population, our estimates imply that the statistical effects of expansion of employment and training programs (especially employment programs)

TABLE 4.

## AHEM AND CPS MEASURES OF EMPLOYMENT, UNEMPLOYMENT AND LABOR FORCE FOR TOTAL POPULATION, 1963-76

(Annual Averages, In Thousands Of Persons)

| | Employment | | | Unemployment | | | Labor Force | | |
|---|---|---|---|---|---|---|---|---|---|
| Year | CPS | AHEM | diff | CPS | AHEM | diff | CPS | AHEM | diff |
| 1963 | 67762 | 67763 | * | 4070 | 4070 | * | 71833 | 71832 | * |
| 1964 | 69305 | 69302 | 3 | 3786 | 3789 | -3 | 73091 | 73090 | 1 |
| 1965 | 71088 | 70957 | 132 | 3366 | 3749 | -113 | 74455 | 74436 | 19 |
| 1966 | 72895 | 72609 | 286 | 2875 | 3109 | -234 | 75770 | 75718 | 52 |
| 1967 | 74372 | 73982 | 390 | 2975 | 3296 | -321 | 77347 | 77277 | 70 |
| 1968 | 75920 | 75522 | 397 | 2817 | 3130 | -314 | 78737 | 78653 | 84 |
| 1969 | 77902 | 77424 | 476 | 2832 | 3205 | -373 | 80733 | 80629 | 104 |
| 1970 | 78627 | 78111 | 516 | 4088 | 4473 | -386 | 82715 | 82584 | 130 |
| 1971 | 79120 | 78538 | 582 | 4993 | 5415 | -422 | 84113 | 83953 | 160 |
| 1972 | 81702 | 80909 | 793 | 4840 | 5441 | -602 | 86542 | 86351 | 191 |
| 1973 | 84409 | 83726 | 684 | 4304 | 4791 | -487 | 88714 | 88517 | 197 |
| 1974 | 85936 | 85247 | 688 | 5076 | 5520 | -445 | 91011 | 90768 | 243 |
| 1975 | 84753 | 83776 | 1006 | 7830 | 8501 | -671 | 92613 | 92278 | 335 |
| 1976 | 87485 | 86388 | 1097 | 7288 | 7992 | -705 | 94773 | 94381 | 392 |

Note: Figures within rows may not sum to total due to rounding. * = negligible.

## TABLE 5.

## AHEM AND CPS MEASURES OF EMPLOYMENT, UNEMPLOYMENT AND LABOR FORCE RATIOS FOR TOTAL POPULATION, 1963-76

(Annual Averages, In Per Cent)

| | Employment-population ratio | | | Unemployment rate | | | Labor force participation rate | | |
|---|---|---|---|---|---|---|---|---|---|
| **Year** | CPS | AHEM | diff | CPS | AHEM | diff | CPS | AHEM | diff |
| 1963 | 55.4 | 55.4 | * | 5.7 | 5.7 | * | 58.7 | 58.7 | * |
| 1964 | 55.7 | 55.7 | * | 5.2 | 5.2 | * | 58.7 | 58.7 | * |
| 1965 | 56.2 | 56.1 | 0.1 | 4.5 | 4.7 | -0.2 | 58.9 | 58.8 | * |
| 1966 | 56.9 | 56.7 | 0.2 | 3.8 | 4.1 | -0.3 | 59.2 | 59.1 | * |
| 1967 | 57.3 | 57.0 | 0.3 | 3.8 | 4.3 | -0.4 | 59.6 | 59.6 | 0.1 |
| 1968 | 57.5 | 57.2 | 0.3 | 3.6 | 4.0 | -0.4 | 59.6 | 59.6 | 0.1 |
| 1969 | 58.0 | 57.6 | 0.4 | 3.5 | 4.0 | -0.5 | 60.1 | 60.0 | 0.1 |
| 1970 | 57.4 | 57.0 | 0.4 | 4.9 | 5.4 | -0.5 | 60.4 | 60.3 | 0.1 |
| 1971 | 56.6 | 56.2 | 0.4 | 5.9 | 6.4 | -0.5 | 60.2 | 60.1 | 0.1 |
| 1972 | 57.0 | 56.5 | 0.6 | 5.6 | 6.3 | -0.7 | 60.4 | 60.2 | 0.1 |
| 1973 | 57.8 | 57.4 | 0.5 | 4.9 | 5.4 | -0.6 | 60.8 | 60.7 | 0.1 |
| 1974 | 57.8 | 57.4 | 0.5 | 5.6 | 6.1 | -0.5 | 61.2 | 61.1 | 0.2 |
| 1975 | 56.0 | 55.4 | 0.7 | 8.5 | 9.2 | -0.8 | 61.2 | 61.0 | 0.2 |
| 1976 | 56.8 | 56.1 | 0.7 | 7.7 | 8.5 | -0.8 | 61.6 | 61.4 | 0.3 |

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Note: Figures within rows may not sum to total due to rounding. * = negligible.

## TABLE 6.

## AHEM AND CPS MEASURES OF EMPLOYMENT, UNEMPLOYMENT AND LABOR FORCE FOR POPULATION AGE 16-19, 1963-76

(Annual Averages, In Thousands of Persons)

| | Employment | | | Unemployment | | | Labor force | | |
|---|---|---|---|---|---|---|---|---|---|
| Year | CPS | AHEM | diff | CPS | AHEM | diff | CPS | AHEM | diff |
| 1963 | 4255 | 4255 | * | 883 | 883 | * | 5138 | 5138 | * |
| 1964 | 4516 | 4516 | * | 872 | 872 | * | 5390 | 5390 | * |
| 1965 | 5036 | 4933 | 103 | 874 | 964 | -90 | 5910 | 5924 | 14 |
| 1966 | 5721 | 5494 | 227 | 836 | 1024 | -188 | 6557 | 6595 | 38 |
| 1967 | 5682 | 5379 | 303 | 838 | 1090 | -252 | 6519 | 6570 | 51 |
| 1968 | 5780 | 5481 | 299 | 839 | 1076 | -237 | 6618 | 6680 | 62 |
| 1969 | 6117 | 5803 | 314 | 853 | 1091 | -238 | 6970 | 7046 | 76 |
| 1970 | 6141 | 5795 | 346 | 1105 | 1358 | -253 | 7246 | 7340 | 94 |
| 1971 | 6195 | 5816 | 379 | 1257 | 1532 | -275 | 7453 | 7557 | 104 |
| 1972 | 6722 | 6266 | 456 | 1302 | 1633 | -331 | 8024 | 8149 | 125 |
| 1973 | 7236 | 6859 | 377 | 1225 | 1484 | -259 | 8461 | 8579 | 118 |
| 1974 | 7403 | 7058 | 345 | 1410 | 1628 | -218 | 8813 | 8940 | 127 |
| 1975 | 7046 | 6589 | 457 | 1752 | 2039 | -287 | 8799 | 8969 | 170 |
| 1976 | 7269 | 6767 | 502 | 1701 | 2004 | -303 | 8970 | 9168 | 198 |

Note: Figures within rows may not sum to total due to rounding. * - negligible.

## TABLE 7.

## AHEM AND CPS MEASURES OF EMPLOYMENT, UNEMPLOYMENT AND LABOR FORCE RATIOS FOR POPULATION AGE 16-19, 1963-76

(Annual Averages, In Per Cent)

| | Employment-population ratio | | | Unemployment rate | | | Labor force participation rate | | |
|---|---|---|---|---|---|---|---|---|---|
| Year | CPS | AHEM | diff | CPS | AHEM | diff | CPS | AHEM | diff |
| 1963 | 37.4 | 37.4 | * | 17.2 | 17.2 | * | 45.2 | 45.2 | * |
| 1964 | 37.3 | 37.3 | * | 16.2 | 16.2 | * | 44.5 | 44.5 | * |
| 1965 | 38.9 | 38.1 | 0.8 | 14.8 | 16.3 | -1.6 | 45.7 | 45.6 | 0.1 |
| 1966 | 42.1 | 40.4 | 1.7 | 12.7 | 15.7 | -3.0 | 48.2 | 48.0 | 0.2 |
| 1967 | 42.1 | 39.9 | 2.2 | 12.8 | 16.8 | -4.0 | 48.4 | 48.0 | 0.4 |
| 1968 | 42.2 | 40.0 | 2.2 | 12.7 | 16.4 | -3.7 | 48.3 | 47.9 | 0.5 |
| 1969 | 43.4 | 41.2 | 2.2 | 12.2 | 15.8 | -3.6 | 49.4 | 48.9 | 0.5 |
| 1970 | 42.3 | 39.9 | 2.4 | 15.2 | 19.0 | -3.7 | 49.9 | 49.3 | 0.6 |
| 1971 | 41.3 | 38.8 | 2.5 | 16.9 | 20.9 | -4.0 | 49.7 | 49.0 | 0.7 |
| 1972 | 43.5 | 40.6 | 3.0 | 16.2 | 20.7 | -4.4 | 52.0 | 51.1 | 0.8 |
| 1973 | 46.0 | 43.6 | 2.4 | 14.5 | 17.8 | -3.3 | 53.7 | 53.0 | 0.8 |
| 1974 | 46.1 | 44.0 | 2.1 | 16.0 | 18.7 | -2.7 | 54.9 | 54.1 | 0.8 |
| 1975 | 43.3 | 40.5 | 2.8 | 19.9 | 23.6 | -3.7 | 54.1 | 53.1 | 1.0 |
| 1976 | 44.3 | 41.2 | 3.1 | 19.0 | 22.8 | -3.9 | 54.6 | 53.4 | 1.2 |

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Note: Figures within rows may not sum to total due to rounding. * = negligible.

since 1963 have grown steadily in importance. The effect of employment and training programs on reported levels and rates of unemployment has increased steadily over time; the effect on reported levels of employment and labor force, and on reported employment-population and labor force participation figures, has likewise increased steadily over time.

In view of our discussion in Part II above, this is hardly astonishing; but the magnitudes of these effects may come as something of a surprise. For example, the estimates suggest that, in the absence of employment and training programs, the national unemployment rate would have been about eight-tenths of a percentage point higher in 1976 than the actual figure; and that the teenage unemployment rate would have been about 3.9 percentage points higher than the official 1976 figure. Similarly, the estimates imply that, in the absence of the programs, the aggregate employment-population ratio would have been about seven-tenths of a percentage point lower in 1976 than the actual figure; and that the teenage employment-population ratio would have been about 3.1 percentage points lower than the actual 1976 figure.[6]

Finally, the estimates also shed some interesting light on the experience of the second half of the 1960s. Both overall and for teenagers, unemployment rates hit their lowest level for the entire decade in 1969, and employment-population ratios reached their highest level for the entire decade in 1969. Tables 5 and 7 suggest that the effect of employment and training programs on these patterns was considerable. According to the estimates presented in Tables 5 and 7, almost 30% of the total decline in the overall national unemployment rate between 1964 and 1969 was a consequence of the statistical effects of employment and training programs; and that these statistical effects account for nearly all (90%) of the decrease in the teenage unemployment rate between 1964 and 1969.

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[6] Of course, these are estimates, which, as indicated in n. 2 above, are subject to some measurement error. Accordingly, they should be interpreted with some caution. However, to the extent that measurement errors induce systematic biases, we suspect that they work in a downward or conservative direction. Both because of the nature of the AHEM approach and because 1974-1976 CETA enrollment data used in our calculations almost certainly understate actual average total current enrollment (see n. 2 above), it seems probable that the differences between the official 1976 employment, labor force, etc., figures and those which would obtain in the absence of employment and training programs are larger than the ones given in the text.

Of course, the line between unemployment and absence from the labor force is not always easy to draw, and so some observers--notably Geoffrey Moore--have argued that for many purposes the employment-population ratio is a better measure of labor market behavior than the unemployment rate. In the present case, however, comparison of the employment-population ratio with and without employment and training programs over 1964-1969 tells essentially the same story as the comparison of unemployment rates during the same period. Tables 5 and 7 imply that over 15% of the increase in the aggregate employment-population ratio during 1964-1969 was a consequence of the expansion of employment and training programs, and that over 35% of the increase in the teenage employment-population ratio during 1964-1969 was likewise the result of this expansion.[7]

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[7] That is, the increase between 1964 and 1969 in the reported aggregate employment-population ratio was 2.3 percentage points; but the reported employment-population ratio in 1969 would have been 0.4 percentage points lower than it actually was had there been no employment and training programs, according to our estimates. (See Table 5.) Hence the program's purely statistical effects "account" for 0.4/2.3 = 17.4% of the total increase in the aggregate employment-population ratio during 1964-1969. Similarly, the programs account for 2.2/6.1 = 36.1% of the increase in the teenage employment-population ratio between 1964 and 1969; for 0.5/1.7 = 29.4% of the decrease in the aggregate unemployment rate during these years; and for 3.6/4.0 = 90% of the 1964-1969 decrease in the teenage unemployment rate.

## POLICY IMPLICATIONS

The estimates presented here have an obvious empirical significance; moreover, taken in conjunction with the analysis above (which is presented in more detail in Appendix A), the estimates have a number of implications for employment policy and for employment and training programs as instruments of employment policy.

That there is still debate on these issues might seem somewhat surprising, since, as is well known, the debate was supposed to have been over with more than ten years ago. To be sure, even in the good old days of the 1960s, some advocated employment and training programs as means of effecting structural changes in the labor force and reducing the inflation rate associated with any given unemployment rate. But a second school sought what might be called the lift of a driving demand: they argued for monetary and fiscal measures to expand aggregate demand and move the economy along a negative-sloped Phillips curve, incurring an acceptably small increase in inflation in exchange for a much desired reduction in the unemployment rate. In the end, policy gave much more emphasis to aggregate demand measures than to structural measures, and, as shown in Tables 5 and 7, unemployment rates as reflected in CPS data fell sharply. The proponents of what has now become the new old-time religion of demand expansion maintained that this experience renders a "clear-cut verdict" (Heller, 1966, p. 64) on the merits of aggregate demand measures, making "...it ...as clear today as it can possible be" (Ackley, 1966) that the strategy "worked amazingly well" (Oken, 1976, p. 70): the view that "...the inadequate demand camp was right and the structuralists were wrong" (Ackley, 1966) "...was gloriously confirmed by the ease with which new jobs were created and unemployment diminished in the subsequent expansion of aggregate demand" (Tobin, 1974, p. 16)--thus demonstrating "...that macroeconomics can, black youths aside, achieve full employment" (Samuelson, 1969) and that "the American economy can reach unemployment rates of close to 3% through the use of simple fiscal and monetary policies" (Thurow, 1973, p. 84).

If the debate was supposed to be over, why has it continued? Primarily, it seems, because economists rediscovered supply, and realized that the scissors which the aggregate demand school wielded had only one blade. First came the realization that demand expansion not only reduces unemployment but also raises prices, and that eventually money wages will adjust to catch up with prices--thereby cancelling at least some of the previous gains in employment: the demand-expansion road up the Phillips curve seems, in the long run, to be appreciably steeper and more dangerous than initially thought. More recently has come the realization that government supply-side programs have contributed in no small way to recent gains in a variety of economic indicators--gains which were once thought to be attributable entirely to the expansion of aggregate demand during the 1960s.

Our analysis and estimates bear on this debate in several respects. First, just as other authors have found that various supply-side programs--as opposed to "simple fiscal and monetary policies" as such--made an important contribution to the attainment of various policy goals during the 1960s,[8] we, too, conclude that supply-side (i.e., employment and training) programs account for a not inconsiderable part of the reduction in unemployment rates (and the increase in employment-population ratios) during the 1960s. The "clear-cut verdict" which emerges from a consideration of Tables 5 and 7 is that the purely statistical effects of employment and training programs had an important effect on key indicators of employment and unemployment; and the most interesting hypothesis about aggregate demand which is "gloriously confirmed" by these tables is the proposition that members of the aggregate demand school are no more immune from the *post hoc* fallacy than are other economists.

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[8] For example, Plotnick and Skidmore (1975) find that much of the decrease in the incidence of recorded poverty during the 1960s was the result of the substantial expansion of government transfer programs (rather than expansion of aggregate demand *per se*) during that period. Likewise, Butler and Heckman (1977) conclude that "...the supply side effects of recent policy [such as government welfare and transfer programs] play an important role in explaining the recent measured increase in the ratio of wages and incomes of blacks to the wages and incomes of whites" (p. 235).

Second, our analysis also suggests that greater reliance on employment and training programs as a means of reducing unemployment by shifting the economy's Phillips curve may be preferable to what now appears to be the rather perilous policy of reducing unemployment by relying solely on demand expansion and moving along a given--and potentially quite steep--Phillips curve. Indeed, our discussion of the statistical effects of employment (Type III) programs under the condition of general market clearing or, loosely speaking, "full employment" (see especially Appendix A) implies that expansion of such programs can reduce the reported unemployment rate and increase reported employment even under conditions of general market clearing--when the *only* long-run effect of a once-and-for-all demand expansion will be an increase in the price level, and when monetary and fiscal policies to drive the unemployment rate still lower will generate only an accelerating rate of inflation.[9] Since teenage unemployment, especially among blacks, is high even at "full employment," this is of course particularly important for policies to reduce teenage unemployment.

Finally, our discussion in Appendix A of the statistical effects of employment and training programs under conditions of non-market clearing (or what is usually, somewhat imprecisely, called "below full-employment") suggests that expansion of such programs in conditions of non-market-clearing can be a particularly potent form of demand expansion--one which, moreover, can actually be *more* powerful than conventional monetary and fiscal policies as a means of reducing "deficient-demand" unemployment. The analysis of Part II above leads directly to the conclusion that expansion of employment programs will reduce unemployment and increase employment under conditions of non-market-clearing by *more* than, say, a tax cut which raises effective aggregate demand by the same amount.[10] Indeed, if the government expands employment programs under conditions of non-market-clearing entirely by enrolling previously unemployed persons, it will create at least as many jobs *in the private sector* as a tax cut involving the same dollar outlays[11]--and of course will create additional jobs in the public sector as well.

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[9] Proponents of the "natural rate of unemployment" hypothesis may thus find it of some interest that an expansion of employment programs is a means of reducing the unemployment rate without, at the same time, incurring an accelerating rate of inflation.

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[10] This is evident from Figure 2 of Appendix A. An increase in aggregate demand effected via, say, a tax cut will shift firms' demand from the D' schedule of Figure 2 to the D schedule, and would raise total employment from e' to $e_R^*$. In contrast, an expansion of employment programs which had the same effect on aggregate demand would raise employment *in the rest-of-the-economy sector* from e' to $e_R^*$, and would *raise training sector employment* by an additional amount $dx_e + dx_u$, with the total being larger than that effected by the tax cut.

[11] Of course, this is simply a straightforward extension of the familiar balanced-budget multiplier theorem. (See, for example, Killingsworth and King, 1977, esp. pp. 26-28.) To the extent that so-called "fiscal substitution" effects operate, they simply reduce the *difference* between the effectiveness of employment programs and that of a tax cut. (See, again, Killingsworth and King, 1977, esp. pp.23-25.)

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# APPENDIX A
# A SIMPLE MODEL OF THE STATISTICAL EFFECTS OF EMPLOYMENT AND TRAINING PROGRAMS

To see clearly--albeit in a somewhat intuitive fashion which suppresses various details--the nature of the statistical effects of employment and training programs, and to see the nature of AHEM estimates of these effects, it is useful to consider the case of an expansion in enrollment of a Type III program in the context of a simple model of the labor market. (As is evident from Table 1 and 2, enrollment in Type III programs is by far the largest of that in any program, so the results presented here may be taken as a fairly good indication of the effect of all such enrollments: the effects of Type III programs will swamp the effects of Type I and II programs.) Two "sectors" of the labor market are considered: the "employment and training sector," and the "rest-of-the-economy" sector (or ROE, for short). In terms of our discussion in the text, the "immediate" effects of employment and training programs pertain to the "employment and training sector," while what we have called "induced" effects refer to effects on the ROE. Since the "immediate" effects have been discussed in some detail in the text, we focus here on the "induced" effects of employment and training programs and hence on the ROE sector.

As usual, the supply of labor to the ROE, S, is positively related to the real wage, w, while the demand for labor in the ROE, D, is negatively related to the real wage. Figure 1 presents these two schedules. Now suppose--purely for the sake of argument--that all markets clear and that the prevailing real wage rate is w*, at which supply and demand are equal and equilibrium employment in the ROE is $e_R^*$.[1] Next suppose that the government expands enrollment in a Type III program by an amount dx.[2] This enrollment increase comes from three sources: from persons previously employed in the rest of the economy, $dx_e$; from persons previously unemployed and seeking work in the rest of the economy, $dx_u$; and from persons previously not in the labor force, $dx_n$. So

$$dx = dx_e + dx_u + dx_n$$

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[1] Of course, labor turnover and job search will generate some positive unemployment rate even at this point of general market clearing. See, for example, Hansen (1970).

[2] Presumably, this increase comes about either because the government offers better stipends or training to trainees, or because existing stipends and training were sufficiently attractive to create an excess of applicants over available places which the government now decides to satisfy more fully, or some combination of these two things.

Now, in terms of the model of the labor market of the ROE depicted in Figure 1, expansion of the Type III program shifts the ROE labor supply schedule to the left by an amount equal to $dx_e$--that is, $dx_e$ fewer people are available to the ROE at any real wage in the vicinity of the current level, w*. Hence $dx_e$ vacancies open up in the ROE; labor is now somewhat scarcer there, and so, competing with each other to get more, firms raise wages by an amount dw to a new level, w**, thereby attracting $(\partial S/\partial w)dw$ new workers. That is, having shifted from point 1, the original equilibrium, to point 2 as the result of enrollment increases in the program, the ROE labor market now adjusts to fill some of the vacancies created by that enrollment increase; in doing so, it moves from point 2 to point 3. Thus the *net* change in employment *in the rest of the economy* is only $de_R$, where

$$de_R = -\,dx_e + (\partial S/\partial w)dw$$

while the net change in *total* reported employment--that is, in the employment-and-training sector *plus* the ROE-- is $de_T$, where

$$\begin{aligned} de_T &= de_R + dx = -dx_e + (\partial S/\partial w)dw + dx \\ &= dx_u + dx_n + (\partial S/\partial w)dw \end{aligned}$$

Under the AHEM procedure, however, the total change in reported employment is assumed to be equal to the number of new enrollees who were not employed (i. e., who were either unemployed or not in the labor force) prior to enrollment. In other words, under the AHEM procedure, the estimate of the total change in reported employment is only $d\hat{e}_T$, where

$$d\hat{e}_T = dx_u + dx_n$$

and where, of course, $d\hat{e}_T$ is strictly less than $de_T$: the AHEM procedure yields a conservative (or, in technical terms, downward-biased) estimate of the actual effect on total employment of an expansion of a Type III program.

All this assumes that the ROE labor market does in fact clear. If this is not the case, then, in general, the downward bias in the AHEM procedure is even greater, since this procedure also ignores the multiplier effects which are associated with the program-expansion-induced increase in effective aggregate demand and the resulting return to market-clearing conditions. To see this in an intuitive way, recall that the D curve of Figure 1 is constructed on the conventional textbook assumption that markets do in fact clear and thus that firms are able to sell all they want to at prevailing prices. Obviously, this curve cannot be used to analyze behavior under non-market-clearing conditions. For a very simple model[3] of the labor market under non-market-clearing conditions,

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[3] See, for example, Barro and Grossman (1975). The model presented here suppresses most of the details to be found in their work, but is entirely adequate for our purposes and contains may of the essential ideas to be found in their much more elaborate treatment.

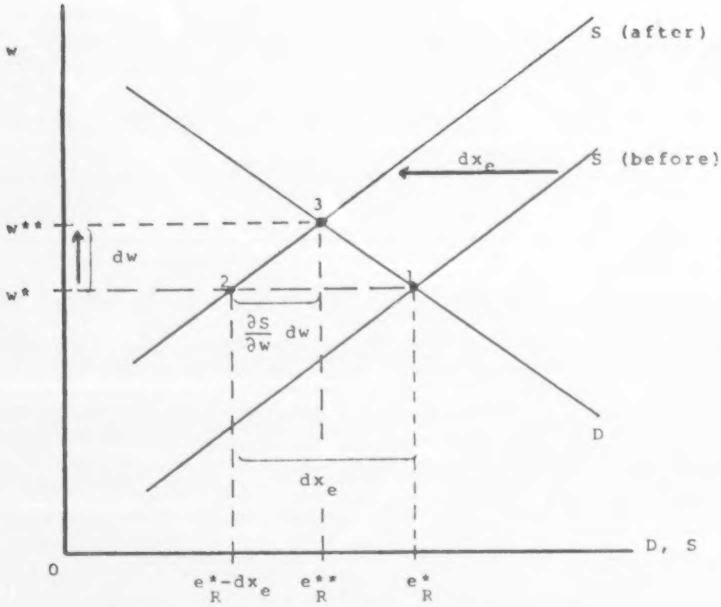

Figure 1. Effect of Type III Program
Under General Market Clearing

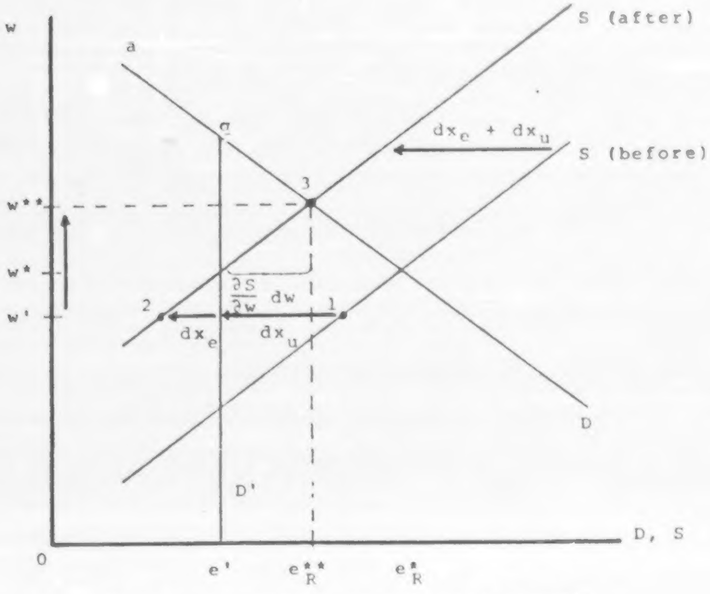

Figure 2. Effect of Type III Program
Under Conditions of Non-Market-Clearing

consider Figure 2. The curves labelled D and S are the same as those of Figure 1--that is, these two schedules represent the demand and supply for labor schedules which would prevail in the ROE if and when firms are satisfied that the labor and commodity markets clear. In this case, the equilibrium real wage would be $w^*$ and the equilibrium level of employment would be $e^*$, as before. However, when markets do not clear, e. g., because the aggregate effective demand for commodities falls short of the output of commodities, firms discover that they cannot sell more than some particular amount of output $y'$ which is less than the market-clearing amount $y^*$. Until conditions improve, they therefore will hire *at most* only $e'$ workers, less than the market-clearing amount $e^*$, because only $e'$ ( $< e^*$) workers are needed to produce $y'$ ( $< y^*$), the maximum amount of output they are able to sell.

Hence, because the commodity market does not clear and firms are output-constrained, the ROE labor demand curve takes the form of the broken line aqe' shown in Figure 2, implying that no more than $e'$ workers will be hired at *any* positive wage. It follows that, even if the real wage is less than the "right" level, $w^*$, firms will not hire more than $e'$ workers, simply because when markets do not clear firms will be unable to sell the additional output which any additional workers would produce. By the same token, "failure" of the real wage to fall is not the reason why the economy does not return to its original position, $e^*$.[4] Of course, competition between workers may drive the real wage down to some extent, though, since this will have no effect on the effective demand for labor, $e'$, nothing will happen to employment in consequence. Rather, the workers must compete for a fixed number of jobs, $e'$.

In any event, suppose that, at present, the real wage is some amount $w'$ as shown in Figure 2; that the government decided to foster a return to general market-clearing conditions by expanding aggregate effective demand; and that it decides to expand demand by expanding a Type III program. The increase in program enrollment shifts the ROE labor supply schedule S to the left by an amount $dx_e + dx_u$ (i.e., the program enrolls some persons who were employed in the ROE prior to enrollment, and also enrolls some persons who were "involuntarily unemployed" in the ROE labor force prior to enrollment). At the same time, the expansion in aggregate effective demand associated with expansion of the Type III program induces firms to switch from the non-market-clearing demand curve D' back to the market-clearing schedule D. Thus, the ROE labor market moves from point 1, the initial employment position, to point 2 due to the enrollment expansion *per se*, and then moves from point 2 to point 3 due to the aggregate demand expansion associated with the enrollment expansion.

In this case, of course, the net change in employment in the rest of the economy, $de_R$, is *positive*, from $e'$ to $e^{**}$. (Note that, by construction, $e^{**} - e'$ is equal to $(\partial S/\partial w)dw$, where $dw = w^{**} - w^*$

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[4] See, for example, Barro and Grossman (1975, especially pp. 61-62).

as in Figure 1. This puts the conclusion in the sharpest possible way, but, of course, the validity of the conclusion in no way depends on it.) Further, the net change in total reported employment, $de_T$--that is, the change in the employment-and-training sector plus the change in the rest of the economy--induced by expansion of the program is even greater, i. e.,

$$de_T = de_R + dx = (e^{**} - e') + dx_e + dx_u + dx_n$$
$$= dx_u + dx_n + (\partial S/\partial w)dw + dx_e$$

But the AHEM estimate of the employment effects of this program expansion is still only

$$d\hat{e}_T = dx_u + dx_n$$

So the downward bias of the AHEM estimate, $d\hat{e}_T - de_T$, is even larger in this case--one of adjustment from non-market-clearing conditions--than it was in the previous case, which assumed general market clearing.

Finally, suppose that employment in the ROE is subject to a minimum wage. (The effect of the minimum wage on aggregate employment may not be very large, but its effect on teenage labor markets may be more important.) Precise conclusions as to the magnitude of the downward bias of the AHEM procedure in the presence of a minimum wage must rest on precise assumptions about various other magnitudes, including (i) the extent to which the minimum wage exceeds the equilibrium or market-clearing level, (ii) the extent to which coverage under the minimum wage law in the ROE is less than complete and (iii) the extent to which expansion of employment and training programs attracts enrollees previously employed in the "covered subsector" of the ROE. However, the *direction* of the bias of AHEM estimates is in no way altered by a *minimum wage*. Indeed, the downward bias of the AHEM procedure will be at an absolute maximum if all employed persons who enter Type III programs come from the "covered subsector" of the ROE. This is because the effect of the minimum wage is to make the supply of labor to the covered subsector infinitely elastic, so that employers in that subsector will fill *all* of the vacancies which will arise when some of their employees depart in order to enroll in a Type III program.

This "induced" effect associated with minimum wages will be somewhat smaller if some of the employed persons entering the Type III program come instead from the ROE subsector that is not covered by the minimum wage law (the "uncovered sector," for short); but even in this case there will still be an "induced" effect of some magnitude. In fact, Figures 1 and 2 and the above discussion of their implications may be taken to refer to the limiting case in which *all* employed persons who enter Type III programs are "recruited" from the uncovered subsector of the ROE, the covered subsector is left undisturbed and the bias in AHEM estimates is therefore at an absolute *minimum*.

Of course, all these results are simply intuitive partial-equilibrium examples rather than rigorous general-equilibrium comparative-statics proofs, and they do not attempt to explore either statistical effects on variables other than employment (e. g., the unemployment rate) or to examine *differential* statistical effects on different labor markets (e. g., for youth vs. other persons). Nonetheless, these results are certainly suggestive; and it is possible to derive these and other results on the statistical effects of employment and training programs--both for "disadvantaged" workers (i. e., the "target" or eligible population group) and for the population as a whole--using a formal model of labor markets and simple comparative-statics techniques.[5] For purposes of this Appendix, we content ourselves with simply stating some of these results, in particular the following general proposition:

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[5] Two important assumptions underlying those derivations which pertain to unemployment seem worth mentioning here. First, we assume that an increase in the general level of real wages increases the labor force by less than it increases the "supply of employment" S (i. e., the number of persons who are willing to accept job offers rather than continue to search), and hence that the number of people who wish to continue to search for work rather than accept work falls as the general level of real wages rises. (Quits vary procyclically, but the duration of unemployment falls as the cycle reaches its peak; and it can be argued that these variables reflect search behavior *per se*. To the extent that this is so, we assume that the former effect is dominated by the latter, generating a negative relation between the real wage level and the amount of "search unemployment.") Second, we assume that enrollment opportunities in employment and training programs are rationed, so that the number actually enrolled is an exogenous goverment-determined policy variable. To the extent that admission into such programs depends on labor force status--e. g., on being or having been unemployed--individuals may deliberately change their labor force status in order to improve their chances of admission; however, to simplify the analysis, we assume that the extent of such behavior is negligible. In fact, the assumption that enrollment opportunities are rationed more or less implies this. Individuals who attempt to improve their chances of admission by becoming unemployed trade off a certain loss of earnings against a highly uncertain and--when enrollment opportunities are rationed--presumably rather small increment in the probability of admission. Some may make this trade-off, but, at least as a first approximation, we assume that the aggregate importance of this phenomenon is trivial.

AHEM estimates of the statistical effects of an expansion of any employment and training program--whether Type I, II or III--are downward-biased, both for "disadvantaged" workers and for the population as a whole.

It is also possible to derive other results which are concerned specifically with the effects of each particular type of program. The results which refer to the statistical effects of Type III programs--which, given the size of these programs, are probably of greatest interest--may be summarized as follows:

If the government expands enrollment in a Type III program, then:

1. Both for disadvantaged workers and in the aggregate, reported employment and labor force levels will both rise and the reported unemployment rate will fall.
2. Both for disadvantaged workers and in the aggregate, the actual increase in reported employment (the reported labor force) will exceed the number of new enrollees who were not employed (not in the labor force) prior to enrollment. Hence, both for disadvantaged workers and in the aggregate, AHEM estimates understate both the employment and labor force increases attributable to expansion of the program.
3. Both for disadvantaged workers and in the aggregate, the difference between the actual increase in reported employment and the number of new enrollees who were not employed prior to enrollment will exceed the difference between the actual increase in the reported labor force and the number of new enrollees who were not in the labor force prior to enrollment. Hence, both for disadvantaged workers and in the aggregate, AHEM estimates understate the reduction in the unemployment rate attributable to expansion of the program.

# APPENDIX B
# DERIVATION OF AHEM ESTIMATES

In Tables 4-7 of the text we present AHEM estimates of the levels of employment, unemployment, etc., which would have prevailed during 1963-1976 in the absence of employment and training programs. These estimates are derived in the following manner.

First, $\hat{E}$, the level of employment which would prevail in the absence of the programs, is assumed to be given by

$$(1) \qquad \hat{E} = E - (0.2-e_{WIN})X_{WIN} + \sum_q X_{Iq}e_{Iq} + \sum_r X_{IIr}e_{IIr} - \sum_s X_{IIIs}(1-e_{IIIs})$$

where E is the level of aggregate employment recorded in official statistics, $e_{WIN}$ is the fraction of enrollees in WIN who were employed prior to enrollment, $e_{pz}$ is the fraction of enrollees in the zth Type-p program who were employed prior to enrollment, $X_{WIN}$ is total WIN enrollment and $X_{pz}$ is total enrollment in the zth Type-p program.[1] Likewise, the number of persons who would be in the aggregate labor force in the absence of the programs is

$$(2) \qquad \hat{L} = L - X_{WIN}(1-\ell_{WIN}) + \sum_q X_{Iq}\ell_{Iq} - \sum_r X_{IIr}(1-\ell_{IIr}) - \sum_s X_{IIIr}(1-\ell_{IIIr})$$

where L is the number of persons in the labor force as recorded in official statistics, $\ell_{WIN}$ is the fraction of enrollees in WIN who were in the labor force prior to enrollment and $\ell_{pz}$ is the fraction of enrollees in the zth Type-P program who were in the labor force prior to enrollment. Together with official data on L and E, data on overall values of X, e and $\ell$ for each program[2] lead directly to a measure of the aggregate unemployment rate which would prevail in the absence of the programs, $\hat{u}$, where

$$(3) \qquad \hat{u} = (\hat{L}-\hat{E})/\hat{L}$$

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1. Note that (1) assumes that 20% of all WIN enrollees are in on-the-job training and similar activities which would cause them to be classified as " employed" by CPS interviewers. Here we follow Small (1972), who, in the absence of reliable data on the composition of WIN enrollees by type of activity, adopted the same assumption. (The limited data available suggest that the 20% assumption is not unreasonable, though it is not possible to be sure whether it is equally appropriate for each year of the WIN and Work Experience programs.)

2. In most cases we have been able to use actual data on X for each

Recall yet again that $\hat{u}$, $\hat{L}$ and $\hat{E}$ are all conservative estimates of the "true" effects of abandoning employment and training programs: that is, $\hat{u}$ is a lower bound on the true unemployment rate which would prevail in the absence of the programs, while both $\hat{L}$ and $\hat{E}$ are upper bounds. This is because, as noted in Part II and Appendix A, the AHEM procedure ignores induced effects (on employment in the rest-of-the-economy sector and via the multiplier) arising from abandonment of the programs. Finally, given data on the civilian noninstitutional population age sixteen and over, P, one may compute estimates of the employment-population ratio and labor force participation rate which would obtain in the absence of the programs, i. e.,

$$\hat{\eta} = \hat{E}/P \tag{4}$$

$$\hat{\lambda} = \hat{L}/P \tag{5}$$

respectively.

Given data on the employment, $E_y$, labor force, $L_y$, and population, $P_y$, of persons age 16-19, one could easily use expressions analogous to (1) - (5) above to compute similar estimates for the youth labor force, if data on program enrollments for youths and the composition of young enrollees by previous labor force status--that is, X, e and $\ell$--were available. Unfortunately, data on e and $\ell$ for persons age sixteen to nineteen (or for any other particular age group) are almost never available. Instead, therefore, we are forced to construct estimates of the direct effects of employment and training programs on youth by assuming that the composition of young enrollees by pre-enrollment labor force status was the same as that of all enrollees. Hence, we compute estimates $\hat{E}_y$, $\hat{L}_y$, $\hat{u}_y$, $\hat{\eta}_y$ and $\hat{\lambda}_y$ for youth, analogous to $\hat{E}$, $\hat{L}$, $\hat{u}$, $\hat{\eta}$ and $\hat{\lambda}$, respectively, for the overall population, by inserting $E_y$ and $L_y$ in place of E and L in (1) and (2), respectively, and then multiplying each total program enrollment figure $X_{pz}$ by $y_{pz}$, the fraction of enrollees in the zth Type-p program who were between sixteen and nineteen years of age.

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program (but see n. 2 in the text above). Data on e, $\ell$ and (see below) y for each program are also usually available, but in some cases only for a few selected years. Here, too, we have had to make a variety of assumptions and estimates when the requisite information was unavailable. For purposes of the present paper, suffice it to say that in this case, as in most other empirical work, the results are subject to an unknown, and probably unknowable, degree of measurement error.

# SOCIAL DEVELOPMENT AND EMPLOYMENT: AN EVALUATION OF THE OAKLAND YOUTH WORK EXPERIENCE PROGRAM

By: Delbert S. Elliott and Brian A. Knowles

## ABSTRACT

This report on the evaluation of the Oakland Youth Work Experience Program presents data from an experimental study incorporating a pretest and two follow-up interviews occurring at six-month intervals after the pretest. For most participants, these data represent the attitudes and perceptions of youth after six months and one year in the program but prior to entry into regular jobs or work careers. The results, therefore, are limited to this short-term impact of the program and may reveal little about its impact following termination from the program or during a subsequent work career.

In general, there was no empirical evidence of favorable program impact on participants (experimentals) during the first six-month evaluation period, i.e., there were no significant differences favoring experimentals on any of the twelve impact measures at the first follow-up. There was limited evidence that participation for a full year had some beneficial effects for experimental respondents. In particular, Powerlessness was found to decline between the first and second follow-ups and Self-Esteem appeared to increase between the pretest and the second follow-up.

In general, control respondents showed little change during the first six-month evaluation period and appeared to show some evidence of unfavorable change by the time of the second follow-up, particularly an increase in negative Parent/Child Relationships.

A large proportion of controls reported some work experience during both evaluation periods. About 75% worked during the first evaluation period and about 50% did so during the second period. Program participants did especially well when compared only with controls who did not work, primarily because the latter showed consistent negative change. Controls who did work did at least as well as experimental respondents. For both program participants and controls, favorable change on the impact scales was systematically related to their perceived satisfaction with their jobs.

Tests of the theoretical model upon which the evaluation was based revealed substantial support. Changes on the impact variables were predictive of changes in Self-Reported Delinquency.

The results from the general analysis were somewhat disappointing from the perspective of the program. There was no evidence that participation in the YWEP resulted in positive change relative to no participation. In the restricted comparisons involving only experimentals with matched controls who did not work, the program (i.e., work) appeared to have some positive effects, although this was primarily the result of declining levels on the various measures for controls rather than gains for experimentals. There was thus some support for the positive impact of work *per se* as suggested in the theoretical model. It must also be anticipated that some of the benefits of program participation may yet

be realized. As of the time of the final follow-ups, most of the youth had been unable to make practical use of their training and experience.

## INTRODUCTION

A review of published research in the area of youth employment reveals numerous studies examining the effects of personality and social characteristics of individuals upon their work choices, career patterns, job satisfaction, absenteeism, morale, and so forth. Likewise, much research has examined the effect of participation in Manpower programs on subsequent earnings (see Rawlins, 1972 for a review). It is interesting to note, however, that few studies have examined the impact of employment upon the social development of youth. Shore (1972), in his review of studies evaluating this latter relationship, cites only five empirical studies and concludes that little is known about how particular work experiences impact upon the developmental process. While several additional studies could be included in a current review (Hackler, 1966; Robins, 1969 and 1974; Walther and Magnusson, 1969; Ahlstrom and Harighurst, 1971; Jeffery and Jeffery, 1970), we still know relatively little about the impact of employment on the social development of youth.

The few studies focusing upon the relationship between work and social development have failed to provide any compelling evidence for the postulated positive effects of work on the attitudes, perceptions, goals or values of youth. It is not the case that all studies have reported negative findings (see, for example, Gartner, et al., 1971; Massimo and Shore, 1963; Walther and Magnusson, 1969; Engel, et al., 1967; Kohen and Parnes, 1971), but rather that the theoretical and methodological adequacy of the research casts doubt upon the validity or generality of these findings whether positive or negative with respect to the postulated impact of work on social development. Methodologically, sample sizes were often small, few studies employed control groups, and none involved an experimental design with random assignment.[1] Given these limitations and the inconsistency in findings, little can be concluded regarding the impact of work on youth development.

Of equal importance in the evaluation of past research is the almost total absence of any explicit theoretical rationale which ought to guide the design and implementation of the work program and link specific program components to evaluation outcomes or objectives. In the absence of specific postulated outcomes based upon some theoretical model or perspective, it is difficult to interpret the empirical findings of evaluation studies. In most instances, a very broad range of "possible," "hoped for" outcomes are identified or implied by program personnel with a forced post hoc rationale, or a general search for "positive" outcomes of any kind is initiated.

In the recent past, the rationale or justification for work programs has focused increasingly upon a reduction in crime or delinquency as the general program objective, but the theoretical connection between involvement in a work program and delinquent behavior is seldom explicit, or involves very crude notions such as the assertion that work keeps youth occupied, reducing the *time* that is available for engaging in delinquent behavior. Apart from the obvious possibility that the work setting may afford *new* opportunities for crime, studies utilizing this theoretical rationale seldom make any deliberate attempt in the structure of the program to maximize time involvement, the critical intervening variable in the connection between work and delinquency reduction; and given the very limited time involvement typically required of participating youth, there is no reasonable justification for assuming that the program should be effective in reducing delinquent behavior. In most instances, these intervening variables involve such things as attitudes toward the norms or authority, goals or aspirations, perceived opportunities, and values, i.e., social development variables.

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[1] The Robin's (1969, 1974) study is presented as involving an experimental design with random assignment, but does not achieve this objective. The random assignment involved youth on a waiting list who were eligible for participation in the In-School NYC Program (Experimental Group 1). These persons were randomly assigned to a Summer Only Program (Experimental Group 2) and the Control Group. No matching prior to assignment was involved, and the Control Group constitutes an adequate comparison group only for the Summer Only Experimentals. Yet, the major comparisons and conclusions involve the In-School Experimentals. It is also the case that the Initial (pretreatment) measures for the In-School Group occurred sometime after their entry into the program, not before. There was also a serious loss of cases across time, with no documentation on the possible bias this loss might have on the results presented. Nevertheless, this study is one of the better studies available on the impact of work on attitudes, perceptions, and behavior (including police contacts for delinquent behavior).

Finally, evaluation studies utilizing reduction in delinquency as the program objective have uniformly failed to measure delinquency in a way which is consistent with their theoretical rationale (even though crude). With few exceptions, work experience is postulated to impact upon delinquent behavior, yet evaluation studies consistently utilize police contacts/arrests as the measure of delinquency. A reduction of police arrests may or may not reflect an actual reduction in delinquent activity; likewise, an increase in arrests does not necessarily reflect an actual increase in delinquent behavior.

Given the theoretical and methodological difficulties noted above, the present state of knowledge regarding the impact of employment upon the social development of youth continues to be very limited. The study described here involved a deliberate attempt to deal with these deficiencies in both the structure and rationale for work experience programs and the methodological adequacy of the evaluation.

## THE OAKLAND YOUTH WORK EXPERIENCE PROGRAM[2]

The Oakland Youth Work Experience Program (YWEP) was based conceptually upon a theoretical model developed by the Office of Youth Development (OYD), DHEW and included selected elements of both the In- and Out-of-School Neighborhood Youth Corps (NYC) program and the Vocational Exploration in the Private Sector (VEPS) program. It was developed as a model program for the U.S. Department of Labor and the initial contract included funds for a comprehensive, long range evaluation of its effectiveness in realizing its stated youth development goals. Only a brief description of the conceptual model and the actual program will be presented here.[3]

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[2] The National Office for Social Responsibility (NOSR) developed this program as a model youth work experience program based on the OYD strategy for youth development (Gemignani, 1971) under a contract with the U.S. Department of Labor, Offender Rehabilitation Division (No. 99-4-0009-013).

The basic premise of the OYD strategy for youth development is that a satisfactory pattern of physical, social and psychological development represents the most effective deterrent to delinquent behavior. An analysis of the developmental processes which result in normative, constructive, prosocial behavior patterns for most youth suggested three major patterns: (1) law-abiding youth are involved in meaningful, legitimate, and satisfying social roles and perceive that they have access to similar roles as adults; (2) these young people believe that their parents, friends, teachers and employers view them positively and they view themselves positively; and (3) they are accepted and integrated into their families and communities, and perceive a high degree of control over both their present and future lives. The development model thus incorporates these three elements as basic to a satisfactory process of youth development and postulates that they insulate youth from involvement in serious or repetitive patterns of delinquent behavior. The youth development model is presented schematically in Figure 1.

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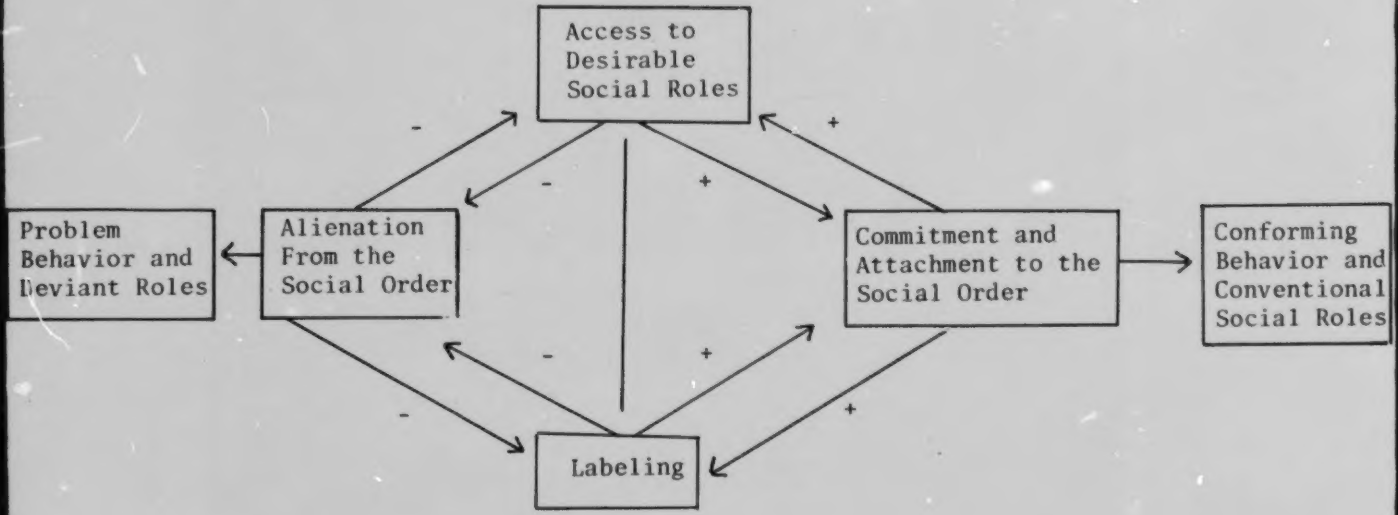
3. For a more detailed description of the theoretical model, see Elliott, et al., 1976; Brennan and Huizinga, 1976; CAR, 1971; Polk and Kobrin, 1972; Gemignani, 1971. For a detailed description of the Program, see NOSR, 1976.

FIGURE 1


Access to Desirable Social Roles
Problem Behavior and Deviant Roles
Alienation From the Social Order
Commitment and Attachment to the Social Order
Conforming Behavior and Conventional Social Roles
Labeling
-
-
+
+
-
+
-
+

+ = high access, positive labeling

- = low access, negative labeling

## Desirable Social Roles--A Stake in Conformity

One of the reasons that many youth do not engage in deviant behavior is that they have little to gain and much to lose by doing so. Youth who are loved and respected at home, successful at school, comfortable among their friends and look forward to a worthwhile work career put all of these in jeopardy if they become involved in delinquent activities. Participation in meaningful social roles, therefore, serves to insulate youth from involvement in delinquent behavior by providing them with a stake in conventional roles and behavior. What is notable about these positive social roles is that they are provided by a handful of institutions--primarily the family, school and work. In large measure, then, we look to these institutions to ensure that youth are provided the opportunity to achieve desirable social roles.

In practice, most youth do experience meaningful involvement and a favorable course of development within these institutional settings. Yet for some youth, these same social contexts systematically limit or deny them access to desirable roles: they are not loved and respected at home, are not successful at school and do not hold much hope for rewarding work careers. Consequently, they may feel that they have no stake in conformity, and that they place little or nothing in jeopardy if they experiment with illegal forms of behavior. In fact, certain types of criminal activity may offer the only hope they see for financial and material rewards.

## Negative Labeling

Once an individual is tagged or defined as a "troublemaker," "a truant" or "a delinquent" others tend to view him as such and to treat him according to this label. When parents, teachers and friends and employers begin to use such labels as a basis for their interaction with a person, the individual is under great pressure to define himself/herself in a similar way and to behave in a manner which is consistent with this definition and the social role it implies. At this point, the individual has become what he has been labeled, thereby confirming the original definition. The fact that this was a self-fulfilling prophecy, and that those participating in the labeling process contributed to the youth's becoming a troublemaker, truant or delinquent, often goes unnoticed.

Of particular concern are the negative labeling processes which occur in the school, at work and in the juvenile justice system. Because the negative labels employed in these institutional settings are more visible and formal than those generated elsewhere, they tend to have a greater effect on an individual's life. Being defined as a troublemaker at school, for example, has an impact not only on the courses one takes (vocational as opposed to college bound) but also on participation in extracurricular activities, assignment to particular teachers, and even seating locations within the classroom--all of which impinge upon an individual's future educational and occupational opportunities.

The application of the label "juvenile delinquent" to youth processed in the juvenile justice system has similar negative effects. Often these youth find themselves cut off from contact with conventional, law-abiding youth and thrust into association with youth who are committed to delinquent roles; their future educational and occupational opportunities are diminished by their having an official record; and friends, parents, teachers and significant others in their lives begin to view them as being "different" and to respond to them selectively in terms of the delinquent label. The labeling process becomes complete when, as a consequence of this new definition and the resultant expectations of others, the youth comes to view *himself* as a delinquent person.

The problem with such institutional labeling is that it is often discriminatory and inappropriate, dictated by stereotypic attributes or system requirements rather than by a careful evaluation of an individual's abilities, values and commitment to particular kinds of behavior. The danger of labeling, then, lies in the very real possibility that while the youth has not make any commitment to the specific behavior which generated the label, the labeling process itself may serve to reinforce the very behavior which was viewed as being objectionable.

Alienation

Limited access to meaningful, responsible and satisfying social roles and negative labeling result primarily from child-rearing practices at home and institutional processing procedures at school, work and in the juvenile justice system. Alienation represents a type of individual response to such experiences within these institutional settings. Difficulties at home, failure at school and little hope for a rewarding work career result in many youth feeling defeated and rejected. They feel that they have no stake in these institutions and, consequently, that they have no reason to be committed to the appropriate rules of conduct within these settings. In its broadest sense, alienation denotes a destruction of one's affective ties to the social order: a weakening of one's feelings that he or she belongs and is a part of the family, the school or the community; that he or she is morally obligated to obey the rules; or that there are any positive rewards for striving to do what is right. In essence, then, alienation is a rejection of one's rejectors--a psychological disengagement from institutions and the general social order which they support.

The significance of this type of response to negative labeling and limited access to desirable social roles at home, school and work is that it constitutes a psychological form of "permission" to ignore or violate the rules. If one feels that he or she doesn't belong, has no possibility of realizing any rewards from continued involvement and, ultimately, has no moral obligation to those in authority in these institutions, then he or she is free to engage in any forms of behavior which are personally gratifying. There is no investment and, hence, nothing to lose.

The model depicted in Figure 1 is a dynamic model in which feedback relations reinforce the basic processes. On the deviant side of the model, negative labeling and limited opportunities lead to alienation which in turn accentuates and compounds the negative experiences. Ultimately, this leads to deviant roles and a dependence upon illegitimate opportunities or means for attaining social rewards. The objective of youth development programs such as the Oakland Youth Work Experience

Program is to intervene in and disrupt the process by increasing access to desirable social roles, by reducing negative labeling and alienation, and ultimately by reinforcing conforming or prosocial behavior patterns.[4]

## THE YWEP PROGRAM

The goals of the project are based upon the youth development strategy discussed above. The intended program outcomes are thus: (1) to increase youth's access to socially acceptable and meaningful roles both at school and in relation to present and future work roles; (2) to reduce negative labeling of youth at home, school and work; (3) to reduce feelings of alienation and rejection and, as a consequence, (4) to reduce involvement in delinquent behavior.

The target population for the Oakland YWEP involved delinquent and pre-delinquent youth aged sixteen to eighteen. Delinquent youth were those who had been adjudicated as such by official court action. Pre-delinquent youth were defined as those whose behavior might ultimately lead to court action if unchecked. In addition, it was necessary that program participants meet poverty level guidelines as specified by the Office of Management and Budget.[5]

Three methods were used to obtain applicants for the program. First, the county probation department was asked for referrals of delinquent youth. Second, a number of community agencies such as schools, recreation departments, churches, health facilities, and others were asked for referrals of predelinquent youth. And third, all of the available public media were used to attract youth (especially dropouts) not known to existing agencies. Since there were many more applicants than positions available, a selection procedure (described in a later section) was developed to determine which youth were to be admitted to the program.

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[4] This model is derived conceptually from several longstanding theoretical perspectives on delinquency, particularly opportunity theory (Cloward and Ohlin, 1960; Elliott and Voss, 1974), control theory (Hirschi, 1969) and labeling theory (Lemert, 1951; Becker, 1963; Schur, 1971 and 1975).

[5] In fact, the proportion of adjudicated delinquents among participants and controls was small (less than 10%).

At the start of the program, special teams were formed consisting of twenty-five youth participants, a member of the project staff and assigned volunteers. The intent of the teams was to develop a greater intimacy for the program to provide more special attention to the specific needs of each participant and provide a positive social setting and group support for each participant. The team worked together during orientation to emphasize continuing self-assessment and individual and team responsibility for decision making.

In addition to providing work experience, the Oakland YWEP program also attempted to meet other basic needs of its participants including the provision for improvement of basic educational skills and requirements necessary in the world of work. Participants in the Oakland project were given classroom training designed to fill gaps in their formal training. Following an assessment of each participant's mathematics, English, and reading ability, an individualized educational plan was developed. The plans included participation in accredited programs offered by area high schools, adult education programs, public schools, a street academy, and a local community college. The purposes of educational plans were to give youth the opportunity to participate in a curriculum designed to be relevant to career possibilies, to increase their perception of career opportunities, and to improve their chances of work success, while obtaining educational credits toward their program participation.

The work stations that were made available to youth participants represented a wide variety of occupations offering career opportunities and skill development. In general, they were intended to increase perception of the realities of the work world and to provide meaningful, satisfying work experiences. More specifically, the objectives of work experience were (1) to provide experience in occupations in short supply and which, if possible, relate to the occupational goals and capabilities of the participants; (2) to provide a range of work experiences from general exploratory work to beginning and intermediate skill levels to advanced skills; (3) to provide opportunities for maximum skill acquisition commensurate with participant ability and taking account of time limitations; and (4) to provide an opportunity to experience new kinds of relationships with others in a work role, aided by advice of counselors, site supervisors, and participant teams.

The final phase of the program involved intensive preparation related to a participant's placement goals. Youth were given assistance and training in the preparation of resumes, applications, and entrance tests. If the youth desired job placement, a wide range of full-time employment opportunities were explored and every effort was made to place the youth in a permanent job. Others were motivated toward careers requiring further specific skill acquisition, and contracts with the appropriate institutions were established for the youth. For those who developed an interest in completing formal educational goals, assistance was given in terms of curriculum development and sources of financial assistance. Finally, information and orientation were provided for those who became interested in enlistment in one of the military services.

In short, participating youth were prepared to compete more effectively for work roles and provided some first hand experience in positive work roles through a comprehensive program involving need assessment, educational training, work experience, and finally career placement. In the process, youth completed approximately 675 hours of paid participation about equally divided between classroom training and work experience.

## PROGRAM EVALUATION

The evaluation was intended to provide an assessment of the effects of the YWEP experience on participating youth. As the Oakland program had been organized around a broad strategy for the development of prosocial behavior in disadvantaged delinquent and predelinquent youth, the focus of the evaluation was derived primarily from that conceptual model. Thus, the variables of the conceptual model described above and the relationships among them were examined in detail.

## Research Design

The research design employed for the Oakland YWEP evaluation is depicted in Figure 2. It is a classic experimental design with matched experimental and control groups and both pre- and post-tests on the dependent variables. The experimental treatment in the present research is participation in the work experience program.

As can be seen in Figure 2, one post-test was planned at six months after entry and a second post-test was planned at twelve months after entry. Following six months in the work experience program, experimental participants were selected for one of three alternate paths. That is, they were: (1) continued in the work experience program for an additional six months, (2) admitted to an Action Program involving public sector jobs, or (3) terminated from the work program.

In the notation of Campbell and Stanley (1966) the design would appear as:

$$\begin{array}{ccccc}
\underline{t_1} & & \underline{t_2} & & \underline{t_3} \\
0_1 & X & 0_2 & X & 0_3 \\
0_4 & X & 0_5 & A & 0_6 \\
0_7 & X & 0_8 & & 0_9 \\
0_{10} & & 0_{11} & & 0_{12}
\end{array}$$

where X is the work experience program described in detail above, A is the Action work program, and the $0_i$ are the various administrations of the Impact Questionnaire; $t_1$ is May-June, 1975; $t_2$ is December-January, 1976; and $t_3$ is June-July, 1976.

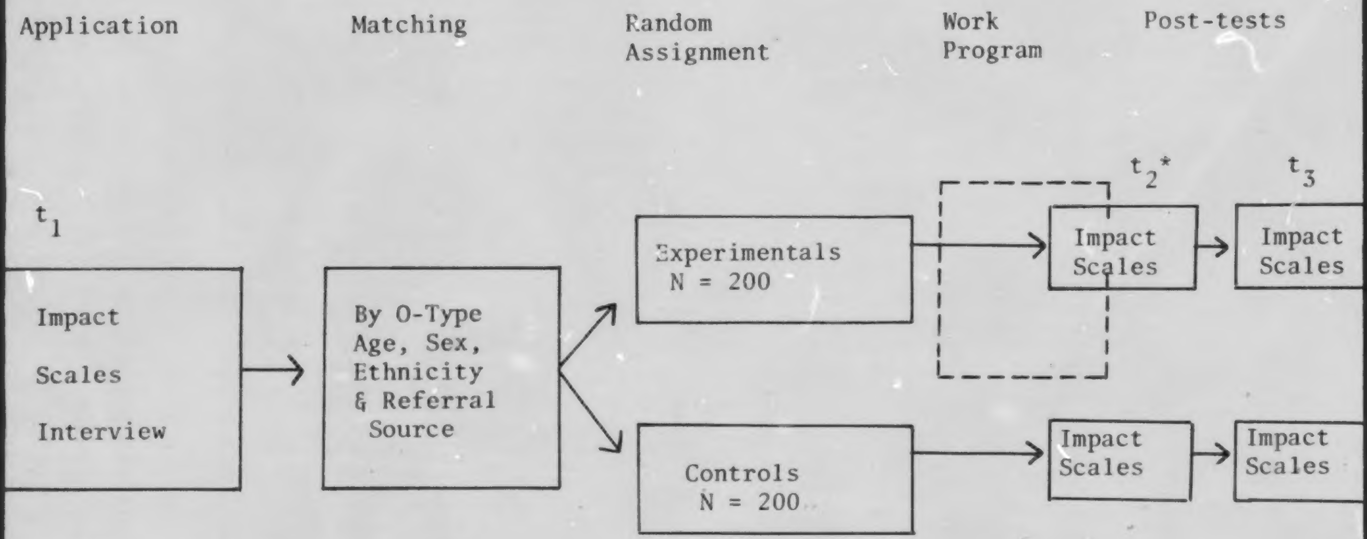
## Participant Selection and Assignment

As described earlier, applicants for the program were solicited from several sources including referrals from the probation department, from other public and private agencies, and through advertisements in the mass media. If a youth met eligibility requirements (age, family income, and residence) he was asked to complete an application. Youth submitting an application were told that there would likely be many more applicants

FIGURE 2

OAKLAND YOUTH WORK EXPERIENCE PROGRAM

RESEARCH DESIGN


Application
Matching
Random Assignment
Work Program
Post-tests
$t_1$
Impact Scales Interview
By O-Type Age, Sex, Ethnicity & Referral Source
Experimentals N = 200
Controls N = 200
$t_2$*
$t_3$
Impact Scales
Impact Scales
Impact Scales
Impact Scales

* $t_2 - t_1 = 6$ months

$t_3 - t_2 = 6$ months

than positions in the program. Those accepted would, therefore, be randomly selected by computer and the chances of being selected were about one in three. About 600 applications were received.

Each youth was given application and parental permission forms. When these were returned, the Impact Questionnaire (evaluation instrument) was administered. This questionnaire includes a number of items concerning social and demographic background in addition to twelve social psychological scales.

The Impact Questionnaire was self-administered in a room with as many as ten youths at one time. Applicants were specifically assured that their responses to the evaluation instrument would in no way affect their chances of selection for the program. Staff members were present throughout to answer questions.[6]

Using a typological analysis procedure, youth were matched in pairs. The members of each pair were highly similar on all twelve scale scores and were identical in terms of age, sex, ethnicity, and referral source (probation referral vs. other source). By this procedure, 202 matched pairs were obtained. Each pair was then split randomly, one member being assigned to the experimental group and one to the control group. The experimental group was then admitted to the work program.

## Impact Scales

Before proceding to the analysis, a brief description of each of the twelve impact scales is presented.[7]

1. Access to desirable social roles. Three scales were used to assess this component:

a. Perceived access to desirable educational roles is a five-item scale intended to measure perceived likelihood of achieving educational goals. A respondent is first asked how far he would like to go in school and then is asked the five questions concerning the probability of attaining this or similar goals. The higher a person's score on this scale, the higher is his perceived chance of obtaining the desired educational goal.

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[6] When necessary, a staff member was present who was fluent in both English and the respondent's native language. In about twenty cases, the entire questionnaire was read to a respondent.

[7] The actual scale items and the documentation of psychometric properties of each scale are available and may be obtained by writing to the authors.

b. Perceived access to desirable occupational roles is a six-item scale intended to measure perceived likelihood of achieving occupational goals. Again, a respondent is first asked what kind of job he would like as an adult and then is asked the six questions relating to the probability that this or similar goals will be reached. A higher score on this scale reflects a higher degree of perceived access to occupational goals.

c. Parental/child roles is a five-item scale measuring a youth's belief that his parents are interested in him and would help him if he needed it. The scale represents a measure of access to desirable social roles for youth in the home. The lower a respondent's score on this scale, the better is his perceived social role in his family.

2. Negative labeling. Four scales were used to assess different dimensions of perceived negative labeling:

a.-c. The content of these three perceived negative labeling scales is identical, but the reference point varies (parent, teacher, and peers). In each case, the scale is a six-item measure of perceived negative or antisocial categorization by significant others. The scale consists of six semantic differential dimensions with a seven-point continuum. The respondent is to indicate where on each dimension he feels he is seen by his parents (or teachers or peers). A higher score on these scales implies a higher degree of perceived negative labeling.

d. Self-esteem is measured with a ten-item scale similar to Rosenberg's (1965) scale. It assesses the extent to which a youth values, accepts, and respects himself. The focus is on self-acceptance. A higher score on this scale reflects a higher level of self-esteem.

3. Alienation. This component of the conceptual model was assessed with three scales, each measuring a different dimension of alienation:

a. Normlessness is measured with a six-item scale designed to assess the extent to which an individual believes that socially unapproved behaviors are required to achieve given goals. A higher score indicates a greater level of normlessness.

b. Powerlessness, a twenty-one-item scale, is a version of a scale developed by Nowicki and Strickland (1973). This scale measures a youth's sense of control over events in his life, his feelings of power over activities and circumstances which affect him. A higher score implies a higher degree of powerlessness.

c. Societal estrangement is assessed with a sixteen-item scale measuring the extent to which a youth feels estranged or alienated from the larger society. The present scale is a modified version of a scale of anomie developed by McClosky and Schaar (1963). The goal of the modifications was to make the scale more suitable to youth. A high score reflects a high degree of societal estrangement.

4. Normative pressure from peers. This is an eight-item scale intended to measure the extent of pressure towards conforming or deviant behavior felt by a youth from his friendship group. It reflects the type of peer group environment in which the individual participates and indirectly reflects his social role in the larger adolescent/peer culture. A higher score on this scale reflects a higher perceived pressure from peers to be deviant.

5. Self-reported delinquency. One scale was used to measure participation in delinquent roles. This is a 19-item scale of self-reported delinquency. The scale is an adaptation of the Nye-Short (1957) Delinquency Checklist. The higher one's score on this scale, the higher the amount of delinquency reported.

## ANALYSIS--IMPACT ON YOUTH

### 1. Initial Comparisons

The primary analysis utilized in the evaluation of the Oakland YWEP involved simple t-tests comparing the experimental and control groups. Statistical tests comparing experimental and control scale scores at the first administration of the Impact Questionnaire revealed no significant differences. This was to be expected since these groups were matched at that time; nevertheless, the tests verified the initial assignment process. Since there were no differences at time 1, experimental-control comparisons at subsequent times were made directly on the raw scores. That is, it was unnecessary to compute gain scores or rely on analysis of covariance.

### 2. First Follow-up Comparisons

Six months after the initial administration, the first follow-up Impact Questionnaire was administered. The timing of this post measure coincided with the completion of the participants' initial six months work experience program and the administration of the interview schedule was included as part of a general exit interview for all experimentals. For controls, personal interviews were arranged, in which controls were asked to come to the project office and complete the questionnaire. They were offered a $2.50 payment as an incentive to complete this follow-up schedule. For experimental youth, the questionnaire was identical to that administered at the beginning. For the control group, a number of items were added for the first follow-up. These additional items related to possible work experiences occurring since the youth applied to the project so as to allow for the control of this potentially contaminating influence on experimental-control comparisons. Overall, 189 of the 202 experimentals (94%) completed the first follow-up interview schedule compared to 152 of the 202 controls (75%).

The first analysis undertaken involved a test for selective loss of either experimental or control participants. Statistical tests comparing the initial scores of the thirteen experimental cases who were lost at time 2 with the corresponding scores of the 189 respondents who were retained revealed no significant differences (t-tests). On only one scale (Access to Educational Roles) did the difference even approach conventional levels of significance.

The analysis comparing the initial scores of the 50 control cases who were lost at time 2 with the corresponding scores of the 152 cases that were retained revealed several significant differences. Respondents who were lost at time 2 were lower on both Access to Education Roles and Access to Occupational Roles and higher on Negative Labeling by Peers.

Overall, there were relatively few differences found here indicating selective mortality. Nevertheless, the three differences found among the control respondents all tended to favor those who were retained at time 2. That is, those who were retained had more desirable scores at time 1. The possibility of a slightly biased control sample must be accepted. For t-tests comparing experimentals and controls, the effect of this bias is in the direction of making the tests more conservative.

The next set of analyses was intended to test for changes from time 1 to time 2 in either the experimental or control groups. For experimental cases who were interviewed at both times, four significant differences were obtained over this six month pre-post period. On Normlessness, Access to Occupational Roles, Negative Labeling by Peers and Self-Reported Delinquency, scores increased from time 1 to time 2. Only one of these, the increase in Access to Occupational Roles, can be regarded as a favorable change. For the control cases who were interviewed at both times, only one scale (Negative Labeling by Peers) showed significant change (an increase) from time 1 to time 2. In general, both groups evidenced some negative change on perceived labeling by peers across time. While the experimental group reported an increase in perceived access to occupational goals, they also reported more normlessness and delinquency.

Next, the scores of experimental and control repondents were compared at time 2. For this analysis, only matched pairs at time 2 were included. There were 144 pairs. The results of this comparison are presented in Table 1. Only two significant differences were found, both favoring the

## TABLE 1

## COMPARISON OF TIME 2 SCALE MEANS FOR EXPERIMENTAL AND CONTROL CASES MATCHED AT TIME 2

| Scale | N | Group | $\bar{X}_2$ | Direction of Change from $t_1$ | t-score | P |
|---|---|---|---|---|---|---|
| Normlessness | 142 | Experimental | 12.24 | + | .40 | NS |
| | | Control | 12.11 | - | | |
| Societal Estrangement | 143 | Experimental | 38.19 | + | 2.46 | <.05 |
| | | Control | 36.60 | - | | |
| Powerlessness | 143 | Experimental | 28.59 | + | 1.44 | NS |
| | | Control | 27.98 | - | | |
| Access to Educational Roles | 143 | Experimental | 11.95 | + | -.31 | NS |
| | | Control | 12.02 | + | | |
| Access to Occupational Roles | 143 | Experimental | 13.54 | + | 1.37 | NS |
| | | Control | 13.16 | o | | |
| Negative Labeling/ Parents | 130 | Experimental | 16.98 | + | .04 | NS |
| | | Control | 16.95 | + | | |
| Negative Labeling/ Peers | 130 | Experimental | 17.42 | + | -.64 | NS |
| | | Control | 17.95 | + | | |
| Negative Labeling/ Teachers | 126 | Experimental | 16.62 | + | .34 | NS |
| | | Control | 16.32 | + | | |
| Self-Reported Delinquency | 143 | Experimental | 23.46 | + | 1.57 | NS |
| | | Control | 22.45 | - | | |
| Self-Esteem | 143 | Experimental | 31.57 | + | -.26 | NS |
| | | Control | 31.68 | + | | |
| Normative Pressure | 142 | Experimental | 13.68 | + | .67 | NS |
| | | Control | 13.44 | - | | |
| Parental/Child Roles | 140 | Experimental | 8.28 | + | 2.47 | <.05 |
| | | Control | 7.64 | - | | |

control group. Experimental respondents scored higher on both Societal Estrangement and Parental/child role. In both cases, the change from $t_1$ to $t_2$ was in opposite directions for experimentals and controls, with experimentals reporting negative gains over time while controls reported positive gains. This explains why significant differences were found between experimentals and controls at $t_2$ on these variables while neither group alone evidenced significant changes across time on these variables.

In general, the results described above are disappointing. There were only two significant changes for either control or experimental cases, and the majority of nonsignificant changes that occurred also favored the control group. In order to investigate the program impact more closely, several further analyses were performed.

One potential source of change resulting from work experience was the youth's aspiration levels. To test this, all of the foregoing analyses were repeated using the respondents' reported levels of Occupational and Educational Aspiration.[8] There was no significant difference between experimental and control cases at time 2 on either aspiration score.

Further analysis of the control data revealed that many of the control cases, while not participating in the YWEP, did obtain jobs on their own in the period between interviews. With respect to the evaluation of the YWEP program, the fact that controls were also working poses no special problem, since the intended comparison is between youth in the program and youth not having benefit of the program and it is assumed that many youth would have obtained work in the absence of the YWEP. To the extent that the study was also concerned with measuring the impact of work per se, the fact that 75 percent of the control cases found work on their own contaminates the general analysis. The theoretical model suggests that to the extent work involves participation in a desirable social role, it should have some direct effect upon perceived negative labeling and feelings of alienation. To evaluate this postulated impact of work, further analyses were performed on the "pure" cases, those matched experimental and control cases in which the control case did not work at all in the interval. There were thirty-five such pairs with complete data.

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[8] Cases lost at time 2 were compared with cases retained at time 2 with respect to these two variables. For neither experimental cases nor control cases was there any difference between cases lost or retained. Further, the experimental cases showed no change on either Educational or Occupational Aspiration from time 1 to time 2. However, the control cases revealed a significant increase ($\underline{t}$=3.18, df=108, $p<.005$) in Educational Aspiration from time 1 to time 2. There was no change in Occupational Aspiration for the control cases.

The experimental cases and control cases were examined separately for changes from time 1 to time 2. The experimental cases showed no significant changes from time 1 to time 2 while the control cases indicated change on two scales over this period. There was a significant decrease on both Normlessness ($t=2.38$, $df=34$, $p<.05$) and Access to Occupational Roles ($t=2.67$, $df=34$, $p<.05$).

The experimental and control cases were also compared on their scores at time 2. Here, three significant differences were found. The experimental cases scored significantly higher at time 2 on Access to Occupational Roles ($t=2.28$, $df=34$, $p<.05$), Educational Aspiration ($t=2.90$, $df=20$, $p<.01$) and Occupational Aspiration ($t=2.59$, $df=20$, $p<.05$). The difference on Access to Occupational Roles resulted from a modest but nonsignificant ($p<.10$) increase for the experimental cases combined with a significant decrease for the control cases (see above). The differences on Educational and Occupational Aspiration were both the result of nonsignificant differences favoring the experimental group at time 1 combined with a nonsignificant increase by the experimental group and a nonsignificant decrease by the control group. These results suggest that there was some favorable impact associated with work which was masked by the general comparison between experimentals and controls.

For controls and experimentals working, there continued to be no significant differences, i.e., there were no observed program effects.

### 3. Second Follow-up Comparisons

The second follow-up questionnaire was administered approximately one year after the initial administration, or about six months after the first follow-up. This administration coincided with the completion of the Extension program or the Action program for those participants and was included as part of the exit procedure. Controls and experimental respondents who were terminated after one six-month cycle of YWEP were contacted in their homes and elsewhere and were paid $2.50 to complete the questionnaire. Although a few new scales were added, all of the basic impact scales were repeated from the earlier administrations. In all, 182 (90%) of the 202 original experimental participants completed the second follow-up interview as did 173 (86%) of the 202 control participants.[9]

The first analysis undertaken involved comparisons from $t_2$ to $t_3$ for the three experimental groups to determine if there was a differential impact associated with these three options, i.e., an extension of the basic YWEP for a second six months, work with Action for a second six months, or exit from the work program after the first six months. While some statistical differences were observed at $t_3$, the analysis revealed that they were the result of selection factors, i.e., the differences existed at $t_2$ and $t_1$ and could not be attributed to differences in the type of work option assigned. Of equal importance, the failure to find differences between the twelve month work options and the six month option suggests that a more extensive involvement with a work program produced no significant gains.

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[9] As with the first follow-up, a test was made for selective loss of participants from either the experimental or the control groups. That is, t-tests were computed comparing the initial scale scores ($t_1$) of the 20 experimental respondents who were lost at the second follow-up ($t_3$) with the initial scores of the 182 respondents who were retained at $t_3$. No differences were found at the .05 level of significance. Similarly, the initial scores of the 29 control respondents who were lost at the second follow-up were compared with the corresponding scores of the 173 respondents who were interviewed at that time. Again no significant differences were found. From these data, it was concluded that selective mortality was not likely a problem in this research. In fact, the retention of 88% of the total population after a full year is considered quite satisfactory.

The next analysis compared the time 3 scale scores of all experimental cases and their matched controls. Again the comparisons were made for Extension, Action, and Terminated experimental cases. The results are summarized in Table 2. These results are striking primarily for the fact that not a single comparison yielded a statistically significant difference. That is, for none of the three groups was there any difference between the mean scale scores of the experimental cases and their matched control cases after twelve months. It is worth noting that for the Action group, the experimental cases scored more favorably than the control cases on twelve of the fourteen measures although the differences were consistently small. While the through time analysis ($t_1$-$t_2$-$t_3$) for experimentals and controls in each of these treatment groups did show some change, it was slight and resulted in no overall differences between experimentals and controls at $t_3$ which were significant.

As was the case in examining changes from time 1 to time 2, the above sets of analyses are generally disappointing, yielding few significant results. Again it was found that many of the control cases had found work on their own during this period, and special analyses were undertaken to isolate the effect of work.

For the first set of analyses, "pure cases" were again selected. These were matched pairs of respondents in which the experimental respondent participated either in the Extension or Action program from time 2 to time 3 and the control respondent did not work. There were thirty-eight such pairs. For these pairs at time 2 there were no significant differences on any scales and the differences that existed were uniformly small. The analysis of change from time 2 to 3 for these cases revealed that experimentals (Extension and Action) showed signifi-

TABLE 2

COMPARISON OF TIME 3 SCALE SCORE MEANS FOR EXPERIMENTAL AND CONTROL PAIRS IN EACH EXPERIMENTAL OPTION

| | | Terminated N=70 | | Action Option N=30 | | Extended Option N=53 | |
|---|---|---|---|---|---|---|---|
| Scale | Group | X | t | X | t | X | t |
| Normlessness | Experimental | 12.09 | -1.60 | 10.93 | -1.64 | 12.54 | .46 |
| | Control | 12.81 | | 11.97 | | 12.32 | |
| Societal Estrangement | Experimental | 36.43 | -1.12 | 35.51 | - .63 | 36.85 | .76 |
| | Control | 37.53 | | 36.48 | | 35.85 | |
| Powerlessness | Experimental | 28.81 | .65 | 26.51 | - .65 | 27.25 | - .54 |
| | Control | 28.47 | | 27.20 | | 27.67 | |
| Access to Educational Roles | Experimental | 11.73 | .73 | 12.06 | - .64 | 11.62 | - .08 |
| | Control | 11.55 | | 12.33 | | 11.64 | |
| Access to Occupational Roles | Experimental | 13.47 | 1.43 | 13.51 | .49 | 13.30 | .61 |
| | Control | 12.97 | | 13.20 | | 13.00 | |
| Negative Labeling/ Parents | Experimental | 17.01 | .30 | 13.82 | -1.17 | 15.88 | - .79 |
| | Control | 16.65 | | 15.82 | | 17.14 | |
| Negative Labeling/ Peers | Experimental | 17.46 | .07 | 15.31 | - .06 | 17.43 | - .71 |
| | Control | 17.37 | | 15.42 | | 18.49 | |
| Negative Labeling/ Teachers | Experimental | 15.21 | - .90 | 13.37 | - .33 | 14.68 | -1.09 |
| | Control | 16.23 | | 13.93 | | 16.57 | |
| Self-Reported Delinquency | Experimental | 24.20 | .17 | 21.83 | -1.38 | 25.63 | 1.07 |
| | Control | 24.00 | | 23.30 | | 23.99 | |
| Self-Esteem | Experimental | 31.14 | 1.24 | 33.07 | - .35 | 32.55 | 1.34 |
| | Control | 30.28 | | 33.34 | | 31.29 | |
| Normative Pressure | Experimental | 13.89 | - .81 | 13.85 | - .08 | 14.12 | .54 |
| | Control | 14.22 | | 13.90 | | 13.81 | |
| Parental/Child Roles | Experimental | 8.54 | -1.39 | 7.99 | - .22 | 7.69 | -1.57 |
| | Control | 9.16 | | 8.14 | | 8.48 | |
| Educational Aspiration | Experimental | 2.11 | -1.03 | 2.09 | - .53 | 2.30 | - .16 |
| | Control | 2.32 | | 2.23 | | 2.35 | |
| Occupational Aspiration | Experimental | 3.65 | 1.77 | 2.59 | - .91 | 2.88 | - .56 |
| | Control | 2.76 | | 3.06 | | 3.15 | |

cant changes on both Powerlessness (a decrease) and Access to Educational Roles (also a decrease). Control cases who did not work from time 2 to time 3 yielded a number of significant changes across that time span. In all there were seven significant differences, all unfavorable. That is, Powerlessness, Negative Labeling by Peers and Teachers, Normative Pressure, and Parental/child roles all increased and Access to Educational and Occupational Roles both decreased. In addition, the remaining seven differences, while not significant, were all in an undesirable direction.

The simple comparison of time 3 scores resulted in two significant differences (on Negative Labeling by Peers and by Teachers), both of which favored the experimental group as did all of the remaining non-significant differences.

One other set of analyses was used to examine the importance of working versus not working for the controls. For these analyses, all control cases who worked between time 2 and time 3 were compared with all control cases who did not work, regardless of their associated experimental group. At time 2, only a single significant difference was found (on Normative Pressure, which was higher among those who subsequently did not work). The remaining scales showed no consistent pattern in favoring one group or the other. At time 3, every difference favored the controls who worked, and six differences were statistically significant. The measures yielding significant differences were Societal Estrangement, Powerlessness, Access to Educational Roles, Negative Labeling by Teachers, Normative Pressure, and Parental/child roles.

One final analysis was undertaken to complete the above picture. That is, control respondents who did work between time 2 and time 3 were again compared with their matched experimental cases (Extension or Action groups). At time 3, no significant differences existed between these groups.

### 4. Job Satisfaction and Impact

As part of the time 3 questionnaire, youth in both control and experimental groups were asked to rate their satisfaction with the job(s) they had between time 2 and time 3. Satisfaction was considered in terms of the work itself (fascinating, boring, creative, challenging, etc.), the supervision (impolite, annoying, tactful, etc.), the pay (adequate, insecure, less than deserved, etc.), promotions (opportunity, frequency, etc.) and coworkers (stimulating, intelligent, lazy, etc.). Scores were obtained on each of these five satisfaction dimensions and a total of all five was also computed. These six scores were then correlated with the raw gain from time 2 to time 3 for each of the fourteen impact measures. Correlations were obtained separately for control respondents and for experimental respondents.

Most of the correlations were quite low, but a number did attain statistical significance. Furthermore, virtually all of the significant correlations were in a direction which reflects the fact that a desirable change on the impact dimensions was related to greater job satisfaction. Among the controls, for example, change on Normlessness correlated -.35 with Satisfaction with the work itself. That is, the greater the satisfaction with intrinsic aspects of one's job, the greater the relative decline in Normlessness from time 2 to time 3.

For controls, satisfaction with the work itself was the satisfaction dimension most often related to change on the impact scales, consistently being associated with favorable change on those scales. The satisfaction dimension least often related to change on the impact scales was satisfaction with promotions. This is reasonable since it would be much less salient in the six month period of interest than any of the other dimensions, all of which would be manifest virtually every day.

Several departures from the general pattern should also be noted, First, increased Satisfaction with pay was associated with a relative increase in Normlessness (r = .18). Similarly, Satisfaction with promotions was associated with increased Negative Labeling by Peers (r = .23). And third, Satisfaction with pay was associated with a relative decrease in Educational Aspiration. This latter result may reflect the fact that satisfaction with pay could mitigate feelings of the need for further education.

The correlations for experimental respondents indicate somewhat weaker relationships with job satisfaction, perhaps because the work experience programs involved a variety of activities only a part of which was the job itself. For experimentals, however, all significant correlations were consistent with the general statement that satisfaction with a job was related to favorable change on the impact scales.

## 5. Test of the Theoretical Model

Regardless of the impact of the YWEP on the youth development variables, it was desirable to determine whether the conceptual model was valid. That is, was change on Self-Reported Delinquency related to change in the other variables? To test this, a stepwise multiple regression analysis was performed with raw gain scores on Self-Reported Delinquency as the dependent variable and the initial scores and raw gain scores on the other scales as predictors. Groups (experimental or control) and initial and raw gain scores on aspiration level were also included as predictors.[10] These analyses are summarized in Table 3.

While the proportion of variance explained is not as high as reported in previous research with this model (Brennan and Huizinga, 1975), there is substantial confirmation in these data. It appears that predictions for males are slightly better than prediction for females, although, over the twelve months pre-post lag, the level of prediction is more similar.

The introduction of the experimental-control variable in the stepwise analyses, produced no significant increase in total explained variance, indicating that this variable made no unique independent contribution to the prediction of changes in delinquent behavior. This simply confirms the previous analysis. It is important to note, however, that gains in occupational aspirations and access to occupational roles were both predictive of a decrease in delinquent behavior, and both of these variables were associated with work experience in the pure case analysis.

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[10] This analysis was performed with both raw gain scores and residual gain scores with only slightly different results. Only the raw gain analysis is reported here.

## TABLE 3.

## MULTIPLE CORRELATIONS PREDICTING GAINS IN SELF-REPORTED DELINQUENCY WITH THEORETICAL VARIABLES

| | PERIOD | |
|---|---|---|
| | 6 Month Follow-up | 12 Month Follow-up |
| | $R^2$ | $R^2$ |
| Experimental | | |
| Males | .30 | .31 |
| Females | .24 | .19 |
| Controls | | |
| Males | .48 | .46 |
| Females | .09 | .35 |

## DISCUSSION

At the global level, comparing all experimentals and controls, the results of the follow-up analyses revealed no favorable outcomes with respect to the youth development variables which could be associated with participation in the Oakland Youth Work Experience Program. The two statistically significant differences observed in the first follow-up, while substantively small, nevertheless favored controls rather than participants. At the second follow-up, there were no significant differences. In general, experimental respondents did not change relative to the control respondents over the twelve month study period.

Special analyses with a restricted focus upon the impact of work involved thirty-five (time 1-2) and thirty-eight (time 2-3) pairs of experimentals and controls, where controls had no work experience in the particular six month pre-post test period. While the small N's render our findings somewhat tentative, the first follow-up analysis of these "pure" cases did produce the hypothesized favorable outcomes for experimentals (i.e., those in work roles) in three areas--increased educational and occupational aspirations and a perceived increase in opportunities for achieving occupational goals. It is precisely in these areas that we would have predicted the work experience should have had its most immediate effect--on aspirations and perceived opportunities for jobs.

The second follow-up analysis also found significant differences favoring experimentals (less negative labeling by parents and teachers) and every other difference, although nonsignificant, favored the experimental group. This occurred, it was found, not because the experimental cases improved from time 2 to time 3 (in general they did not) but because the control cases who had not worked had shown unfavorable change on every scale, statistically significant change on seven of them. Furthermore, when control respondents who had worked were compared with those who had not worked, every difference favored those who worked, with six differences being statistically significant. Again this was not because the controls who worked had improved. Rather, they had simply maintained previous levels while the controls who did not work reported increasingly negative changes across time. A final pure case analysis comparing experimentals and control pairs who were both working, indicated no significant differences on any of the youth development variables for either follow-up.

Taken together, the special work analyses support the view that work experience per se does have some positive impact, particularly upon job aspirations, perceived future work opportunities, and upon a youth's perception of how teachers and parents respond to him. It should be noted, however, that the relative gains associated with work were not dramatic, and that in absolute terms those with work experience did not generally report gains but maintained initial levels on youth development variables. It also appears to make little difference whether the work experience is obtained through a work experience program with its special attention to skill development and job counseling or through work secured by more conventional processes available to all youth. And finally, the more satisfying the work experience, the greater the impact upon youth development variables. Interestingly, the degree of satisfaction with work was unrelated to whether it was obtained in connection with YWEP or through one's own efforts.

The test of the theoretical model was generally supportive and suggests that programs which are effective in increasing perceived access to constructive, meaningful social roles, and generating more positive labels for youth, will impact upon delinquent behavior. Work, whether secured through normal process or through special work experience programs, appears to constitute such a role for youth.

The above findings appear to question the value of the counseling, tutoring and skill development incorporated into most work experience programs. We are hesitant to draw this conclusion. Even twelve months after entry into the YWEP few participants had made any serious attempt to enter the labor force in a full-time capacity. We are concerned that the length of the follow-up is too short reasonably to expect the full impact of the work experience program to be manifest. This argument appears particularly relevant for the skill development/educational component of the program, for YWEP youth simply have had no opportunity to utilize these skills in an effort to secure, maintain or improve their occupational statuses or develop their work careers. We thus agree with Robin's (1974) assessment that a valid confirmation of work program effectiveness requires a long range follow-up of actual occupational performance in the "real world and in the honest-to-goodness labor market," to determine if the program resulted in lower unemployment rates, more efficient work patterns, higher morale and work satisfaction, more advancements and upward occupational mobility as well as improvement on the youth development variables suggested here. Such an analysis is clearly beyond the scope of the short range evaluation studies done to date.[11]

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[11] Shore and Massimo (1969) report on a five year follow-up, but the sample size is so small (N=10) that no generalization can be made.

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a) allows one to unambiguously assert cause and effect;
b) allows one to measure the net effect of a social program without statistical bias; and,
c) is less expensive per marginal unit of information, that is, a given level of statistical precision can be achieved at a lower cost.

All three of these benefits are crucial for society as it attempts to solve its social problems, but this paper focuses only on the second of the three benefits.

## THE NATURE OF THE PROBLEM

The plight of youth in the market place has been severe for approximately a decade. Research interest has characterized the discussion as one of "transition from school to work." Various programs have been devised or rediscovered as means to aid unemployed youth, such as the Job Corps, The Neighborhood Youth Corps, Career Education, and the programs contained in the Youth Employment and Demonstration Act of 1977. However, while there has been a considerable amount of data generated through research on the "youth program," an equal amount of information has not been concomitantly created. We know very little about the absolute or relative efficacy of most of the programs designed to aid youth having problems in school or the labor market. And, in fact, through the use of longitudinal data, it is just recently that the process of transition from school to work has been adequately described, much less understood. This state of ignorance is all the more distressing when one recognizes that tens of millions of dollars have been spent to dispel this ignorance. Essentially, we know a great deal about the characteristics of youth who are suffering these disabilities but little about behavioral processes or how programs to aid youths are likely to work.

Programs to aid youth have been predicated upon reasonable but essentially untested hypotheses about links among the characteristics and behavior of youth, training processes and labor market processes.

# CONTROL GROUP SELECTION

By: Ernst W. Stromsdorfer and Teh-wei Hu

## ABSTRACT

This paper discusses several aspects of preferred research design. It asserts that the classical randomized experimental design offers the best chance of estimating the unbiased effects of a social program, such as youth training or employment programs. Coincidentally, compared to the costs of non-randomized program evaluations at a given level of statistical significance, the randomized experimental design is less expensive per marginal unit of information gained by non-random methods. Thus, this review paper makes a general plea for more extensive use of the classical randomized experimental design and provides some observations on the relative validity of several criticisms concerning the feasibility, usefulness, and the ethics of random assignment in a social experiment or social program.

In the case of non-random assignment, this paper provides a straight-forward discussion of a method to overcome the problem of selection bias by means of regression techniques. It illustrates solutions to problems of non-random sampling schemes in terms of the problems of errors in model specification and errors in variables. However, due to the lack of data and lack of knowledge on model specification, evaluation results obtained from non-random selection data usually remain inconclusive. Since randomization avoids this problem, a strong case for randomized experimental design remains.

## INTRODUCTION

This paper reviews the current state-of-the-art with respect to the use of randomized assignment in the evaluation of social programs such as the Job Corps or similar employment and training programs. As such, it is not original. We assert at the outset that the classical randomized experimental design offers the best chance of discovering, for instance, why the youth unemployment problem is as it is and what to do about it. Compared to other techniques of analyzing social problems and devising social programs to deal with them, the classical randomized experimental design:

The Neighborhood Youth Corps, for instance, never had a clearly articulated policy statement as to how it was expected to return youth to school or improve their employment prospects. The basic idea underlying Career Education is one of a structural relationship between education in its broadest sense and work in a broad sense which continues throughout most of one's productive life. But the relationship has never been rigorously spelled out. In short, our understanding of the "youth problem" is in relative disarray and beset by more ignorance, ambiguity and uncertainty than is conscionable given the time and resources expended on the problem to date. Part, but not all, of the reason for this can be traced to the use of faulty research design and analysis. An additional share can be traced to a failure to carefully articulate how programs designed to deal with the problems were designed to work. These two phenomena clearly interact.

## THE CLASSICAL RANDOM EXPERIMENTAL DESIGN

The "youth problem" has two components. First, there is the problem of analyzing why youth, and especially black youth, have had such high and persistent unemployment in the present and recent past. Second, there is the problem of devising a set of programs to correct the problem.

Suppose a youth employment or training program is to be evaluated by comparing the experiences of an experimental group of youths who were part of the program to the experiences of a control group of youths who were not. The classical advice of statisticians would be to assign subjects at the outset to the experimental and control groups at random, such that any given subject has the same probability of experimental group assignment as any other subject. Such randomization assures that differences between experimental and control group experiences, beyond those differences attributable purely to chance, are legitimately attributable to the program. In the absence of such randomization, analysts may end up with experimental and control group experiences which systematically differ for reasons other than the program. Given the appropriate data, there are statistical techniques for adjusting away such nuisance differences. But, in practice, given imperfect information, there is virtually never concensus among the experts that any given adjustments are adequate. Without adequate adjustment, estimates of program effectiveness suffer to an unknown degree from selection bias. This selection bias problem has thoroughly bedeviled past program evaluations. Indeed, it has been the main problem of past evaluations. Since randomization avoids the problem, there is a strong case for randomization. The case has been made before, but it deserves repeating.

## RESPONSE TO CRITICISMS OF CLASSICAL RANDOM DESIGN [1]

Resistance to use of the classical randomized assignment to a program treatment has remained curiously persistant and severe. It is instructive to list the arguments tendered against the use of random assignment and to evaluate the relevance of their criticism. Following Boruch, these criticisms can be classed according to the following:

### I. Feasibility and Usefulness

Under this general heading classical randomized experiments are asserted to be:

(1.) virtually impossible to implement in real world settings or impossible to perform for some programs;

(2.) relatively more expensive and time consuming than other equally effective, unbiased methods of analysis;

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[1] These arguments and their discussion are largely drawn from Robert F. Boruch, "On Common Contentions about Randomized Field Experiments," in Gene V. Glass, editor, Evaluation Studies, Review Annual, 1976. Throughout this discussion, we mean "experimental design" to imply the test of a program by means of random assignment of program participants to an experimental and a control group for a given social program. This random assignment may also involve or require random assignment to different mixes of treatments within a program experiment. Those social programs which do not involve random assignment we choose to label as demonstration projects, quasi-experiments or natural experiments.

(3.) unnecessary since other quasi-experimental designs or statistical procedures to estimate effects are just as effective;[2]

(4.) too narrow in scope and inherently neglectful of important process or institutional factors and target groups, and therefore unable to yield results which can be generalized.

(1.) Inability to realistically impletment. As Boruch points out there has been a wide variety of social experiments. Experiments generally must remain relatively simple in order to be manageable conceptually and facilitate clear interpretation. Experiments can nevertheless be made to reflect a policy maker's definition of reality, even though this implies greater complexity. However, complex treatment on a diverse set of target groups cause a multiplicative increase in the sample size. This increases the absolute cost of a study considerably. The sample size requirements alone often cause one to argue that the necessary test is not possible.

Obviously, when the policy maker is confronted with these costs, he is then forced to make choices among the policy questions he seeks answers to, the program treatments he would like to test and the target populations he would like to test them on. This is a painful process since our experience has shown that the practical policy maker, other things equal, would rather not have to make such choices, at least not explicitly. Likewise, the policy maker often does not understand the interrelationships between sample size, expected treatment impacts and the statistical precision of the desired estimates he seeks. Thus, he is bound to become disenchanted with the research process, regardless of whether it is a classical experimental design. He turns naturally, then, to such methods as the case study design which promises a richness of detail for which he has a greater taste and familiarity. But, what he often fails to recognize, or even asserts not to be the case, is that he has opted for much less reliable information on a greater number of facets of his problem rather than more reliable information on a smaller set of more critical facets.

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[2] The later part of this paper will discuss and evaluate this assertion in detail.

We find this situation frustrating since there are certain strategic problems concerning youth unemployment which are readily addressed by an experimental design. First, as indicated above, we would like to know the process and causes of youth unemployment. How might certain social programs affect youth labor market experience, for instance? Economic theory supplies hypotheses concerning the effect of minimum wages and income transfers on unemployment which can be readily tested in an experimental framework. An experimental design to test the disemployment effect of minimum wages is not an institutional or research impossibility except for the fact that the Comprehensive Employment and Training Act essentially outlaws such an effort. Differential subsidies connected with unemployment insurance payments (UI) could be used to test the degree to which UI leads to greater job search, higher resulting wages and other reallocations of one's time among work, leisure, job search and home production. But changes in existing law would be needed to perform such a study.[3]

(2.) Relative Cost. If legal constraints were cleared away, the cost of discovering the effect of different policies on youth unemployment would be relatively small. The policy options to test are relatively few: 1) wage subsidies (to interact with the minimum wage), 2) retraining or education in its various forms and, 3) public service employment (PSE). Likewise, the target groups of greatest interest are well identified and relatively few: the two sexes, several ethnic groups, and two to three different age groups within the youth category. This implies, say, 36 separate treatment, sex, ethnic and age groups. If, on the average, between 500 and 1,000 observations per cell are needed to make acceptably precise statements as to treatment effects, we are discussing a sample size in the neighborhood of 18,000 to 36,000 observations.

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3. To its credit, policy makers in the Department of Labor's Employment and Training Administration attempted to get legal permission to conduct experiments within the CETA program, the Unemployment Insurance and Employment Service programs in the fall of 1976. However, the effort was abondoned in the face of broad-based resistance in the Department as a whole along with the unwillingness to use up political capital on the struggle required to press the issue.

It would be possible to design an experiment that would have no more than three or four interviews required per sample unit. Thus, at about $150 per interview, such a study might cost as much as $21,600,000 for data alone. Analysis costs would add two million or so to the cost. Thus, with a parsimonious design that restricted itself in a disciplined way to major target groups and treatments one would likely be able to ascertain, after three to four years elapsed time, whether training, PSE or wage subsidy was more efficacious in reducing youth unemployment. Many subtle nuances could not be handled in such an analysis, but since we do not know even the broad answers to the above questions, the loss of information on nuances hardly seems critical at this juncture.

What would be the net social value of such an effort? As Boruch points out, there have been few formal analyses of the costs and benefits to society of doing no evaluation compared to performing equivocal evaluations or rigorous tests of social programs. In fact, in the employment and training area there have been no such estimates! Such an analysis would be useful for it would help clarify the difference between 1) cost per unit of information acquired versus 2) the expected cost of information to acquire a given increment of social benefit or improvement in social policy. For the first condition, it is always the case that it will be less expensive to acquire a given datum at a specified level of statistical significance by using a classical random design than by some other more impressionistic method. This is so because a random assignment assures that there is no intercorrelation between program treatment and other independent variables of interest. Thus, fewer observations are needed to achieve a given level of statistical precision. This implies a lower cost (Pitcher, 1978).

An assessment of the second case above depends in part on the weight given to information in the political decision-making process and the reasons why information of a given kind is used. Some policy makers clearly only go through the motions of appealing to rational information in making a given decision. For them, objective information has a weight of zero in the actual decision-making process. In such a case, the method used to acquire the information does not really matter. In other situations, if one wants to establish the general direction of an effect, then non-experimental data is usable. A case in point was the simple two-way cross-classification which showed to Congress that program enrollees in the Public Employment Program (PEP) had higher average economic qualifications than the labor force as a whole. Congress did not know the ultimate extent of substitution of higher for lower employable persons under PEP. It did not need to in order to revise the CETA law in the fall of 1975 to attempt to stop the practice of state and local governments of hiring other than the disadvantaged into Public Service Employment Jobs. The point is that one could have used a classical experimental design to attempt to measure the exact degree of this occupational substitution under different constraints but such information simply was not needed for the immediate purpose. All that was required was simple evidence on the very existence of the phenomenon. The cost benefit ratio in this case was very high. However, when more precise behavioral knowledge is needed, an experimental design with random assignment is the only technique which will provide unambiguous information rather than factoids. A case in point is the attempt to measure the impact of an income maintenance program on work incentives through various data sets such as the Current Population Survey or the Survey of Economic Opportunity. A dozen or more quality studies by highly skilled economists were conducted but with very divergent results (Cain and Watts 1973, Chapter 9). It has only been the several negative income tax experiments which have shown any consistency in their estimates (Watts and Rees, 1977). In the former case, among the studies, measures of income and substitution elasticites not only differed in magnitude but in sign, while, except for problems caused by small samples, the negative income tax experiments report generally consistent estimates across studies for given target groups. Use of the former data is next to worthless for policy purposes since there is no objective criterion for choosing among the contending results. Nor does the accumulated evidence describe a narrow enough range of effects. Thus, while pedagogically the various labor supply studies were of considerable value, their value in aiding specific welfare reform policy was next to useless for they gave no guidance on the possible cost of lost output in setting a given level of income guarantee or tax rate on earnings. While it is possible that such studies, through improved data and statistical techniques, could have reduced the range in the estimates of decreased work effort, the NIT experiments actually achieved the result more quickly for a few millions of dollars of economic costs (as distinct from the transfer payment costs used to conduct the experiments).

(3.) Quasi-Experimental Designs and Statistical Adjustments. A major theme of this paper is that it should be possible to model structurally the behavior which leads to self-selection into a program. This will then remove a major source of bias in non-experimental studies. Though, even then, we should point out, in the absence of a classical experimental design, we cannot logically assert cause and effect between treatment and outcome, but only a correlation.

In the act of operating a program, however, systematic selection biases abound. There is selection bias on the part of program operators. Then there is self-selection bias on the part of the individual program participant. If both these processes could be modeled, then such bias could be eliminated. To model these processes one must gain access to them when a program begins. As a practical matter, this has proven difficult to do. These arguments are carried further in the Regression Model Section below.

(4.) Narrowness of Scope and Lack of Institutional Relevance. Except in the case where it is statutorily prohibited, it should be possible to model the most salient components of a program experimentally and randomly assign individuals and a control group to it. Indeed, some existing programs such as the unemployment insurance or the minimum wage program, inasmuch as they offer almost universal coverage, may be impossible to test using a non-experimental design because it is not possible to establish a comparable group who has not been affected by the program treatment. However, it is possible to use random assignment to test various behavioral effects of, say, the UI program, by randomly varying the amount of unemployment insurance benefits among eligible recipients. There is, of course, no zero treatment group. Everyone receives some level of benefit. However, one can discover the differential effects of UI payments over the range of payment amounts that are politically acceptable. The case of the impact of the minimum wage on youth displacement is similar. Given a specific legislated minimum wage, vouchers which amount to differing wage subsidies could be randomly awarded to all unemployed youth, including vouchers of zero value. Such a scheme, if the traditional vested interests did not find it too threatening, would measure the existence and extent of disemployment effects of the federal or any state minimum wage. As noted above, however, studies of wage subsidies *per se* are currently illegal under CETA.

With respect to training programs we should note that a body of experimental evidence could have largely avoided the curious phenomenon whereby training was considered a panacea for employment problems for a decade before opinion in Washington, D.C. largely reversed itself about 1974 and asserted that such programs did not work. Of course, such reversals of opinion are exactly that, reversals of opinion. The existing body of non-experimental evidence can be interpreted either for or against training. The fact is that the conceptual links between training and its addition to human capital on the one hand and the increase in human capital and its effect on earnings and employment on the other simply are not well understood. A non-experimental model of analysis, however, puts more demand on one's understanding of this conceptual linkage than an experimental design does since there is a greater need for more precise specification of the causal model at hand to overcome potential biases in the estimated results.

Of course, as Boruch and others have noted, this modelling process should not be left to the analyst alone. Rather, the program operator or policy maker must participate in this specification.[4] This statement holds for the development of policy questions asked, for the hypotheses to be tested and therefore for the model to test them with, the target groups and treatments under consideration.

Thus, when properly conducted, the charge that an experiment is too narrow in scope and can't be generalized, or does not deal with the correct target groups or treatments or yields irrelevant results is simply incorrect. Persons are most often incorrectly generalizing from observed behavior wherein program operators, policy makers and program analysts fail to interact properly.

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[4] As a practical matter, this, too, is very difficult to achieve, given the incentives to which policy makers and program managers respond.

## THE ETHICS OF CLASSICAL RANDOM EXPERIMENTS

In the analysis of the effects of employment and training programs, mind and body are seldom placed in jeopardy, at least not directly. Also, a given treatment will not always yield net benefits nor does the denial of a treatment necessarily imply a net loss. Either the experimental or the control group may gain or lose, depending on the outcome of the experiment. Therefore, to the exent that economic quantities are the major matters of concern, provisions can be made beforehand to compensate the losers in an experimental test of a program treatment. Another alternative would be to provide the treatment, should it prove beneficial, to the control group at a later date. However, some economic compensation is still required since the control group will have a shorter period over which to gain the benefits from the program.

Finally, we should point out that it may be equally unethical to create social programs whose objectives cannot be realized due to inappropriate design. This amounts, after all, to subjecting people to social "experiments" indirectly and the dashed hopes or unfulfilled expectations are no less of a cost to those who experience them for all that the programs were put forth with honorable and humane intentions. In either case, people have been manipulated by the system and have incurred costs. This is a common charge made to the "War on Poverty."

Thus, in general, the argument that classical experimental designs are unethical in education and labor market analysis is incorrect. In a study with proper safeguards, the losers and gainers are known, the extent of loss and gain can be known, and society's resources are sufficient to compensate the losers if it chooses to do so.

## PROBLEMS OF SELECTION BIAS IN THE REGRESSION MODEL

A randomized experiment is free of selection bias. Thus, comparisons between experimental and control groups can reveal the true effects of a social program. However almost all past program evaluations such as those of Headstart or the several forms of the Neighborhood Youth

Corps have not been based on randomized data. (However, see Robin, 1969, for a study of the Neighborhood Youth Corps which uses a random assignment design). Though analysis of non-randomized data can be valuable when carried out properly (Barnow, 1972; Goldberger 1972a, 1972b), the results are seldom conclusive due to the doubts about whether selection bias has truly been netted out. The inconclusive Headstart evaluation provides a good example (Campbell and Erlebacher 1970) in which the control group appeared more able than the experimental group because the usual procedures of selection and analysis produced systematic biases in the direction of underestimating the effects of the experimental program. In a statistical framework, these biases can be considered errors in specification and errors in variables (Barnow 1972; Goldberger 1972a, 1972b; Cain 1975; and Heckman 1976), either of which may bias regression coefficients. In the case of errors in specification, as long as the omitted variables are introduced into the equation, the model will be properly specified and the expected value of the error term will be zero, i.e., the regression coefficient which measures program effect will be unbiased. In the case of errors in variables, as long as the regressor and the error terms are not correlated, the regression will be unbiased. In fact, errors in specification and errors in variables may be interdependent in the case of non-random sample selection, as will be shown in this section.

### Errors In Specification

Using a youth training program such as Job Corps as an example, the sources of selection bias and suggestions for obtaining unbiased estimates are presented in the framework of regression analysis. Regression is not the only statistical technique for program evaluation, but it is an especially convenient tool. In mathematical derivation, regression analysis is equivalent to analysis of covariance (Goldberger 1964).

A simple linear model for evaluating the effect of a social program is to let

(1) $Y = \alpha_0 + \alpha_1 Z + \varepsilon$

where Y is post program achievement, Z is a dummy variable for the social program (defined as 1 if a youth received program services and defined as 0 if a youth did not receive program services). The error term captures the errors in measuring Y and other omitted variables. If the sample selection of experimental and control groups is randomized, then Z and $\varepsilon$ are not correlated. Therefore, the estimated value of $\alpha_1$ is the unbaised estimate of the effect of training in Y.

If the control group or the experimental group is not randomly selected, but is selected based on a set of sociodemographic factors such as a set of true ability measurements, then the Z variable and error term $\varepsilon$ in Equation (1) will be correlated. In this case a more accurate model of specification for the social program evaluation must include these variables. For the sake of simplicity, only the true ability variable will be included in the model:

$$(2) \quad Y = \alpha_0 + \alpha_1 Z + \alpha_2 X^* + V$$

where X* is the true ability of a youth and V is a new error term equal to $(\varepsilon - \alpha_2 X^*)$. Assume that once X* is introduced into the equation, there is no correlation between Z and V.

A biased estimate of the program effect could result if the true ability variable is omitted from the estimation of Equation (1) when control groups or experimental groups are not randomly selected. The source of bias is the possible correlation between the training variable Z and error term $\varepsilon$. That is, $\varepsilon$ contains the ability variable X*, which may correlate with training (Z), such that

$$(3) \quad X^* = \gamma Z + e$$

where $\gamma$ measures the degree of association between training and ability, and e is the error term.

Substituting (3) into (2), yields

$$\begin{aligned}(4) \quad Y &= \alpha_0 + \alpha_1 Z + \alpha_2(\gamma Z + e) + V \\ &= \alpha_0 + (\alpha_1 + \alpha_2 \gamma) + (V + \alpha_2 e)\end{aligned}$$

Let $a_1 = \alpha_1 + \alpha_2 \gamma$. The difference between $a_1$ and $\alpha_1$ in Equation (1) is due to $\alpha_2 \gamma$. The relation between treatment and true ability may be analyzed as follows:

i) If $\gamma = 0$, (that is, when program treatment is assigned randomly with respect to true ability), then

$$(5) \quad a_1 = (\alpha_1 + \alpha_2 \cdot 0) = \alpha_1.$$

Therefore, no bias exists. The results of $a_1$ in Equation (4) will be the same as $\alpha_1$ in Equation (1). This is the case of a randomized experiment in which regression technique provides an unbiased estimate of the program effects.

ii) If $\gamma < 0$, that is, when the treatment is assigned to youths of lower ability than those in the control group, then

$$(6) \quad a_1 = (\alpha_1 - \alpha_2\gamma) < \alpha_1.$$

iii) If $\gamma > 0$, that is, when program treatment is assigned to youths of higher ability than those in the control group, then

$$(7) \quad a_1 = (\alpha_{11} + \alpha_2\gamma) > \alpha_1.$$

In this case, regression analysis can lead to a bias in favor of program treatment effects, reflecting a type of 'creaming' or self-selection process such as may occur in various employment and training programs.

The results of the previous three cases imply that there are two approaches to obtaining unbiased estimates of the effect of youth employment and training programs. The first approach is random assignment of experimental and control groups before the program begins to ensure zero correlation between the treatment variable Z and the error term $\varepsilon$ in Equation (1). The second approach, in the case of nonrandom assignment, is to formulate a fully specified regression model, including the true ability variable and other sociodemographic variables on which selection into the program was based to ensure zero correlation between Z and V in Equation (2). To know what kinds of variables are omitted in Equation (1) requires familiarity with program selection procedures. Once the relevant omitted variables are included in the regression model, then errors of specification can be avoided and one source of correlation between the training variable and the error term can be eliminated. However, the problem in practice is that the variable or variables on which selection takes place (the ability variable X* in our simple example) are either not known or are known but have not been measured. The analyst is then forced into procedures analogous to regressing Y and Z as if Equation (1) were the true model when Equation (2) is in fact the true model. The result is a biased estimate of the program's effect. Therefore, under nonrandom selection it is virtually never possible to argue persuasively that an estimated program effect has truly accounted for all variables on which the nonrandom selection was based. The only remaining foolproof option is randomization. That is, randomization negates the bias caused by omitted variables.

## Errors in Variables

When random selection is not possible, three other bases for selection may be used. One is based on true ability, X*; the second is based on achievement, X; and the third is unknown selection.

Suppose that true ability X* is normally distributed, with an expected mean of zero and variance $\sigma^2$:

$$(8) \quad X^* \sim N(0, \sigma_*^2).$$

Furthermore, assume that pretraining achievement X and post-training achievement Y are erroneous measurements of true ability, such that

$$(9) \quad X = X^* + U$$

$$(10) \quad Y = X^* + V$$

Where U and V are normally distributed with zero mean and common variance, $N(0, \sigma^2)$. Let Z be a dummy variable for training as defined before. And, let U be independent of V, $X^*_1$ , and Z, such that

$$(11) \quad \rho(U, Z) = \rho(U,V) = \rho(U,V) = (U,X_1^* ) = \rho(V, X_,^*) = \rho(V, Z) = 0$$

when selection is based on true ability and X* is unavailable for the evaluation--only a pretraining acheivement measure is available. Therefore the regression model becomes

$$(12) \quad Y = \alpha_0' + \alpha_1'Z + \alpha_2'X + V'$$

where $V' = V - \alpha_1 U$. The use of a pretraining achievement measure X for X* is subject to the problem of errors in variables. In regression analysis, errors in variables cause biased estimates, such that $\alpha_1 \neq \alpha_1'$ and $\alpha_2 \neq \alpha_2'$ (as shown in Barnow (1972)).

When selection is based on pretraining achievement, then Z is determined by X. Thus Z depends on both X* and U, but remains independent of V. Pretraining achievement reflects only a part of true ability in so-called incidental selection (Lord and Novick 1968), since the control group does not come entirely from the high ability segment of the population; the control group also includes low-ability individuals who happened to achieve unusually well on a measure of labor market skill. When selection is explicitly based on pretraining achievement (not on true ability) and pretraining achievement is used as a regressor in the regression model,

$$(13) \quad Y = \beta_0 + \beta_1 Z + \beta_2 X + V$$

where X is not correlated with V, then even though the control group is more able, there is no bias in the estimate of the effects of training. Campbell (1969) refers to this type of evaluation as a "regression discontinuity design" and recommends its use when nonrandom selections are made. Goldberger (1972a) has shown this advantage by means of mathematical derivation.

When the selection process is not fully known, there are two ways to eliminate correlation between Z and V in order to obtain unbiased estimates of the effect of training. One way is to rely on labor market behavior and economic theory to provide relevant control variables which may correlate with the training variable as well as the post training achievement variable, as suggested by Cain (1975). This approach may reduce the size of the error term and eliminate correlation between Z and V. The other approach is to consider the training variable Z itself as an endogenous variable which correlates with V. Therefore, Z can be estimated as a function of a set of relevant selection factors which are not correlated with V. Once $\hat{Z}$, the estimated probability of participation in the program, is obtained and used as a regressor, the regression estimates of the training effect should be consistent (Heckman 1977).

This is an instrumental variable approach. It is known that although this approach provides consistent estimates, the empirical results of this approach are difficult to interpret.

In summary, the most desirable approach to evaluation of a social program is the randomized experiment. When random assignment is not possible, proper specification of the evaluation model is necessary in order to ascertain the unbiased effects of a program. However, to repeat, due to the lack of data and lack of knowledge of the selection process, evaluation results obtained from nonrandom sample selection data are inconclusive. Therefore, it is important to encourage evaluators and econometricians to pay more attention to the problem of sample selection bias, so that the results of program evaluation can be useful and meaningful to policy makers.

## SUMMARY

This paper discusses several aspects of proper research design, namely, the proper procedure to conduct a social program evaluation. It makes a general plea for more extensive use of the classical random research design, presents the standard criticisms of such a design, and provides some observations on the relative validity of these criticisms. Within this context, this paper has discussed the merits of past studies on the evaluation of manpower training programs, with special reference to the nonrandom sample designs.

This paper provides a simple version of dealing with problems of selection bias in a regression model framework. It illustrates problems of the nonrandom sampling scheme in terms of the problems of errors in model specifications and errors in variables. It is hoped that future program evaluation will rely upon randomized experimental design so that evaluation bias can be minimized. This will provide more positive knowledge about ultimate program impacts and do so at lower social cost than has been the case to date.

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# METHODS OF ALLOCATING FUNDS TO ALLEVIATE TEENAGE UNEMPLOYMENT PROBLEMS

By: Joseph J. Cordes & Robert S. Goldfarb

## ABSTRACT

This paper examines methods for allocating funds currently available for combatting youth labor market problems. Since available studies disagree about the nature of the teenage unemployment problem, we propose allocation methods consistent with various alternate views: (1) that teenage unemployment has only short-run effects on earnings; (2) that it affects future (lifetime) earnings and achievement. The implications of a cohort-specific versus a continuing teenage unemployment problem are also considered.

Our analysis suggests that decisions about program mix and eligibility requirements should be made separately from decisions about allocation of funds to areas. Both program mix and fund allocation decisions are likely to vary depending on whether teenage unemployment is seen as having short-term or long-term effects. Specific allocation formulas are proposed that take these distinctions into account.

We then compare recommended program mix and fund allocation procedures with those found in the Youth Employment and Demonstration Projects Act. Several discrepancies emerge. First, the Act does not ensure uniform program mix across areas, even though uniform program mix may be desirable under certain views of the teenage unemployment program. Second, the statistical indices by which funds are allocated in the Act frequently differ from those recommended. However, conceptually different indices may produce similar empirical results. We therefore compare the distribution of funds by state under the Act with the distribution of funds under various recommended allocation schemes. Regional distributions are shown to vary considerably under the actual and recommended distributions.

## INTRODUCTION

Continuing concern over youth unemployment has led to Congressional funding of programs to alleviate youth labor market problems. The Youth Employment and Demonstration Projects Act of 1977 will provide $1.5 billion for this purpose, and future Congresses may provide additional funding. How should these funds be allocated to obtain the greatest benefit per dollar spent? It is tempting to approach this important policy question by turning immediately to available statistics. There are any number of statistical sources--youth unemployment and employment rates, youth labor force and population statistics, poverty data, and so forth--which seem to provide relevant information. This temptation should be avoided, however, since statistical series viewed in isolation, without an analytical framework, will not generally provide the best possible answers.

There are at least two reasons why the statistics alone are insufficient. First, different statistical series may suggest alternative allocations of funds. For example, if states are ranked by youth unemployment rates and poverty rates, the Spearman rank correlation coefficient between the two rankings is quite low.[1] This indicates that the geographical distribution of youth employment and training funds would be rather different if such funds were allocated by youth unemployment rates rather than poverty rates.

Second, and more fundamentally, the underlying problem must be carefully analyzed if policy goals are to be specified concretely. For example, suppose the goal is to ameliorate the "bad effects" of teenage unemployment. If teenage unemployment implies higher adult unemployment when the cohort of teenagers ages, the goal of attacking youth unemployment may actually be to prevent adult unemployment in the future. If teenage unemployment does not lead to higher adult unemployment, then the goal may be to prevent juvenile delinquency, provide income earning opportunities to members of low income families, and so forth.

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[1] This correlation was calculated from 1970 Census Data and data on youth unemployment rates for twenty-five states presented in U.S., Congress, Congressional Budget Office, Policy Options for the Teenage Unemployment Problem, Background Paper No. 13. (Washington: Government Printing Office, 1976), p. 92.

Section I of the paper explores the nature of the youth employment problem and evaluates its public policy implications. This analysis is used in Sections II, III, and IV to derive and evaluate allocation schemes based on various statistical indicators.

## I. DIAGNOSIS OF THE PROBLEM AND POLICY IMPLICATIONS

Certain basic features of the teenage unemployment situation are illustrated by Table 1. Table 1 presents teenage and adult unemployment rates for 1950-75. Since the 1950s the teenage unemployment rate has been between 2.5 and 4.7 times the adult rate. Moreover, the ratio of teenage to adult unemployment in the late 1960s was higher than in preceeding and following periods. Furthermore, nonwhite youth rates have consistently been higher than rates for white youths. Since the labor force participation rate in the 1970s was considerably lower for nonwhite male youths than for whites, the measured racial unemployment differential probably understates the true differential.

### EXPLANATIONS OF TEENAGE UNEMPLOYMENT

These basic findings have been subjected to different interpretations. Three recent analyses represent a range of informed opinion about teenage unemployment. We briefly review these analyses and then discuss their relevance to allocation decisions.

The first analysis is Martin Feldstein's.[2] In his view, chronic high youth unemployment is due to two main causes: (1) slow absorption of new entrants into the labor market, and (2) low job attachment among young workers. A relatively high percentage of youthful job seekers are new entrants who must spend some time in job search before obtaining their first employment. In 1975, roughly 36% of unemployed teenagers

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[2] Martin Feldstein, "The Economics of the New Unemployment," The Public Interest, vol. 33 (Fall 1973), pp. 3-42.

## TABLE 1

## UNEMPLOYMENT RATES FOR TEENAGERS COMPARED WITH NON-TEENAGE UNEMPLOYMENT RATES IN THE UNITED STATES, 1950-1975

| Year | (1) Age 16-19 | (2) Age 20+ | (1) - (2) Difference | (1)÷(2) Ratio |
|---|---|---|---|---|
| 1950 | 12.2 | 4.8 | 7.4 | 2.5 |
| 1951 | 8.2 | 3.0 | 5.2 | 2.7 |
| 1952 | 8.5 | 2.7 | 5.8 | 3.1 |
| 1953 | 7.6 | 2.6 | 5.0 | 2.9 |
| 1954 | 12.6 | 5.1 | 7.5 | 2.5 |
| 1955 | 11.0 | 3.9 | 7.1 | 2.8 |
| 1956 | 11.1 | 3.7 | 7.4 | 3.0 |
| 1957 | 11.6 | 3.8 | 7.8 | 3.1 |
| 1958 | 15.9 | 6.2 | 9.7 | 2.6 |
| 1959 | 14.6 | 4.8 | 9.8 | 3.0 |
| 1960 | 14.7 | 4.8 | 9.9 | 3.1 |
| 1961 | 16.8 | 5.9 | 10.9 | 2.8 |
| 1962 | 14.7 | 4.9 | 9.8 | 3.0 |
| 1963 | 17.2 | 4.8 | 12.4 | 3.6 |
| 1964 | 16.2 | 4.3 | 11.9 | 3.8 |
| 1965 | 14.8 | 3.6 | 11.2 | 4.1 |
| 1966 | 12.8 | 2.9 | 9.9 | 4.4 |
| 1967 | 12.9 | 3.0 | 9.9 | 4.3 |
| 1968 | 12.7 | 2.7 | 10.0 | 4.7 |
| 1969 | 12.2 | 2.7 | 9.5 | 4.5 |
| 1970 | 15.2 | 4.0 | 11.2 | 3.8 |
| 1971 | 16.9 | 4.9 | 12.0 | 3.4 |
| 1972 | 16.2 | 4.5 | 11.7 | 3.6 |
| 1973 | 14.5 | 3.8 | 10.7 | 3.8 |
| 1974 | 16.0 | 4.5 | 11.5 | 3.6 |
| 1975 | 19.9 | 7.3 | 12.6 | 2.7 |

SOURCE: Columns (1) and (2), Bureau of Labor Statistics, from the data base of Data Resources, Inc. (DRI).

were new entrants to the labor force.

Low job attachment among young workers offers an explanation of youth unemployment based on the unattractiveness of jobs rather than their unavailability. Entry level jobs for teenagers often offer neither valuable training, nor opportunites for advancement. As a result, employers are less reluctant to lay off young workers when demand falls because no investment has been made in these workers. Moreover, young workers are more likely to leave jobs perceived as dead-end, particularly if comparable jobs are readily available.

Feldstein also argues that the "true" social and economic problem of teenage unemployment is overstated by existing data. First, much of the movement of youths both between jobs and into and out of the labor force merely reflects the fact that attending school is the major activity of roughly 40% of the labor force aged sixteen to twenty-one years.[3] Students typically look for full-time employment in the summer and part-time jobs during the school year. Feldstein argues that omission of part-time workers would substantially lower the unemployment rate calculated for sixteen to twenty-one-year-olds.[4]

The problem of teenage unemployment may also be overstated as a result of differences in attitude and motivation between youths and adults. Most young workers do not have the same family responsibilities as older workers; and they may desire more leisure than is consistent with full-time employment and permanent attachment to a firm. Hence, teenagers are more likely than adults to alternate between working and other activities. This tendency may have been heightened by rising wage rates which "permit a comfortable standard of living with less work, or less responsible work than was possible twenty years ago."[5]

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[3] Ibid., p. 13.

[4] Ibid.

[5] Ibid.

In contrast to Feldstein's analysis, Edward Kalachek points to labor supply factors as causes for the recent increase in teenage unemployment.[6] In 1955, the ratio of the teenage to the adult unemployment rate was 2.8 while in 1969 this ratio had risen to 4.5. Kalachek argues that this deterioration in the relative labor market position of teenagers is due to a bulge in the teenage population.

Between 1960 and 1971, the teenage population rose by approximately 50%. In the absence of instantaneous labor market adjustments, this increase in the supply of young workers would result in higher teenage unemployment. Moreover, certain aspects of the youth labor market would slow the process of adjustment. Specifically, Kalachek argues that the problem of increased supply is aggravated by rising school enrollments. Because students seek part-year or part-time work, the standard job is not appropriate to their needs. This need for "different" jobs constrains the ability of labor markets to adjust rapidly to teenage population growth.

However, with time, employers would adjust their production processes and hiring procedures to take advantage of the ready availability of young workers at reduced wages. Kalachek argues that this process should soon be completed if only because teenage population will stop increasing. Teenage population will fall from 10.7% of the population in 1975 to 10.1% by 1980. However, he cautions that teenage unemployment will remain higher than adult rates in the new "post-population bulge" equilibrium. This is largely because the involvement of teenagers in school activities results in higher rates of labor force entrance and reentrance than among adults.

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[6] Edward Kalachek, Labor Markets and Unemployment (Belmont, Ca.: Wadsworth, 1973).

A third analysis of the teenage unemployment problem stresses the impact of the minimum wage on the employment opportunities of teenagers. Recent econometric studies provide consistent evidence that teenage employment has been decreased by the minimum wage, but studies focusing on unemployment give ambiguous results. Two explanations may be offered for these results. First, if the minimum wage discourages labor force participation, it is possible for employment to decrease while unemployment remains unaffected. Second, Goldfarb has argued that the ambiguity is due to the difficulty of controlling for population shifts (such as Kalachek's supply bulge).[7] Indeed, in a number of statistical studies, population shifts seem to have an independent effect on unemployment, but this may in part be due to a (hidden) minimum wage effect.[8]

In addition to direct effects of minimum wages on teenage employment, there are some indirect effects. Feldstein, for example, argues that minimum wages prevent employers from hiring teenagers for jobs with significant on-the-job training potential because they must be paid the minimum wage immediately even though they have not acquired necessary on-the-job training.[9] This restricts the number of jobs with "career potential" available to teenagers, and hence contributes to the job attachment problem mentioned by Feldstein as a cause of higher teenage unemployment rates.

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[7] Robert S. Goldfarb, "The Policy Content of Quantitative Minimum Wage Research," in Industrial Relations Research Association, Proceedings of the Twenty-Seventh Annual Winter Meeting, December 1974, pp. 261-268.

[8] The statistical studies indicate that unemployment increases are correlated with increases in teenage population. Suppose these teenage population increases are treated as outward shifts in the supply curve of teenage labor. Such outward shifts would not necessarily lead to rises in unemployment in the absence of a minimum wage. Without a minimum wage, wages might adjust downward to clear the market, so no extra unemployment would result.

[9] Feldstein, op. cit., p. 15.

None of the above analyses focus on the distinction between white and nonwhite teenage unemployment problems. Indeed, Iden has pointed out that reasons for somewhat different levels and cyclical patterns of white and black teenage unemployment are not well understood.[10] Locational differences, for example, do not seem to explain as much of the black-white unemployment differential as one would expect.[11] Moreover, the falling participation rate of black youths has not been well explained.

## IMPLICATIONS FOR ALLOCATION CRITERIA

These analyses suggest two distinctions relevant for allocation criteria. The first distinction is between immediate and lifetime effects of teenage unemployment. If a teenager experiences unemployment today, that probably lowers family earnings today. However, unemployment may also increase the teenager's probability of unemployment (or more generally lower chances for career achievement) in later life.

The "cocoon" view of teenage unemployment stresses the immediate rather than lifetime impact of teenage unemployment. Under this view, teenage unemployment is high because a high proportion of teenagers are in school and looking for part-time work unrelated to future career plans. Even those not in school may not have established independent households or acquired family responsibilities. For those workers, a serious commitment to work and career may not yet have been developed. However, just as the caterpillar turns into a butterfuly, these teenagers will become serious workers once they leave school, get married, and otherwise settle down.

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[10] George Iden, "Business Conditions, Demography, and the Teenage Unemployment Problem," unpublished paper presented at the Annual Meeting of the Southern Economic Association, New Orleans, November 3, 1977.

[11] Ibid., p. 20.

In contrast, the "hangover" view of teenage unemployment stresses lifetime impacts. Under this view, teenage unemployment results in missed opportunities to learn good job habits and to obtain valuable training. Apart from any immediate impact on family income, teenage unemployment results in diminished future opportunities for unemployed youths.

The analyses summarized above may be interpreted in terms of this distinction. Feldstein's analysis is fundamentally ambiguous, containing elements consistent with both views. On the one hand, teenagers are not able to obtain enough jobs with career potential, implying that teenage unemployment has lifetime impacts. On the other hand, teenage unemployment is attributed to "special" characteristics of young workers, such as school attendance and a greater desire for leisure, which presumably will change as young workers mature into adult workers. Under this view, the effects of teenage unemployment are more immediate than lifetime. Kalachek's "supply bulge" explanation does not really address the "cocoon/hangover" distinction apart from suggesting that impacts, whether immediate or lifetime, are likely to be concentrated on the group of teenagers that comprise the population bulge. Finally, the minimum wage explanation is consistent with both views. To the extent that a higher minimum wage limits employer provision of training, there is a definite hangover. On the other hand, if the minimum wage primarily eliminates part-time jobs with little training component, the bad effects of teenage unemployment are more likely to have an immediate, but not a lifetime impact.

A second important issue concerns the existence of specific cohort effects. Is high teenage unemployment (with or without hangovers), expected to attack a particular cohort especially severely? While Feldstein's analysis does not really address this issue, the other two analyses certainly do. Kalachek's supply bulge analysis clearly implies that "excessive" teenage unemployment is cohort-specific; once the population bulge passes, unemployment rates will return to a lower "equilibrium" level. The minimum wage analysis contains a similar implication. We argued above that whatever the independent effects of population increases, they are likely also to intensify minimum wage effects on employment. Thus, if there is an unusually large cohort of teenagers, the minimum wage effects are likely to be unusually large.

## II. ALLOCATION SCHEMES CONSISTENT WITH DIFFERENT VIEWS OF TEENAGE UNEMPLOYMENT

In this section we suggest allocation schemes that take account of the hangover-cocoon distinction and the cohort-specific versus nonspecific distinction. The desired scheme varies with one's view of the nature of the teenage unemployment problem. Once these various "ideal" allocation criteria have been described, we compare them to the allocation rules of the Youth Employment and Demonstration Projects Act (YEDPA).

Suppose that the "cocoon" diagnosis of teenage unemployment is deemed to be the appropriate one; teenage unemployment has current effects on the teenager's income and that of his family, but has no impact on his future earnings or career pattern. Since unemployment as a teenager has short run rather than lifetime effects, allocation of funds should be aimed at ameliorating these short run effects. A central feature of the short run effect is loss of income, but a plausible argument is that, from society's point of view, this income loss is more serious the lower the income of the teenager's family. This suggests that funds be allocated primarily to aid lower income families.

In distributing funds, the decision maker is faced with two conceptually distinct allocation problems. The first involves allocating funds to different programs so as to achieve a desirable program mix. The second involves allocating funds to different geographic areas once program mix has been determined.

Consider first the problem of determining program mix. Presumably there exists a menu of available programs, each of which constrains participation in various ways through eligibility requirements. Since the distribution of program benefits by income class may differ for programs with different eligibility rules, it is appropriate to choose a mix of programs that achieves a desired distribution of benefits by income level. Once this desired program mix is chosen, each region should be required to use programs in that specified mix. Given similar regions, this procedure would help assure that the desired income distribution of clients was achieved in each region.

An example may be useful. Consider the case where all competing programs are equally effective at increasing teenage incomes; that is, $1 of program cost raises teenage income $X for each program. Then if an estimate of the income distribution is available for each program, determining the income distribution of benefits produced by various program mixes is straightforward.[12] Once alternative distributions are calculated, the one "closest" to the desired distribution can be picked.

When different programs are differentially effective at raising teenage incomes, choice of mix is more complex. If the desired distribution pattern can be obtained for a program mix using only the most transfer-effective programs (those that transfer the most income to teenagers per dollar of expenditure), then this is the preferred solution. If, on the other hand, the desired mix cannot be obtained using only the most transfer-effective programs, then alternative mixes using less efficient programs must be considered. If any of these produces a more desirable distribution of benefits than the best mix of efficient programs, the decision maker must choose between "efficiency" (more income transferred to teenagers) versus "equity" (a more desirable distribution of the dollars transferred).[13]

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[12] For example, consider two programs, each of which raises teenage income $X per $1 spent. Program 1 gives a% of this $X to teenagers with family incomes between $0 and $4000, b% to teenagers with family incomes of $4000 to $8000, and c% to teenagers with family incomes greater than $8000, where a+b+c=100%. Program 2 transfers g%, h%, and i% to the same groups. Then if, for each $1 spent on program 1, $R are spent on program 2, the distribution to the three income classes per dollar spent is $\frac{a+Rg}{1+R}$ to the lowest income class, $\frac{b+Rh}{1+R}$ to the middle income class, and $\frac{c+Ri}{1+R}$ to the highest income class.

Once desired program mix is decided upon, the second allocation problem must be faced; the allocation of funds to areas must be determined. Since the aim is to allocate funds disproportionately to unemployed teenagers in low income families, a useful procedure is to allocate funds to areas in proportion to the number of unemployed teenagers in poor families. This has the attraction of allocating toward the index--low income--which the programs are trying to bolster.

Suppose teenage unemployment is viewed as having long run rather than just short run effects. In this case, how is the allocation decision to be made? Once again it is useful to distinguish between program mix decisions and funds allocation decisions given program mix.

In designing youth employment programs, one is faced with a tradeoff between the goals of immediate job creation and future employability. Emphasis on future employability as opposed to job creation would seem to favor programs that devoted resources to structured work, training, and supervision for participants. Since the costs per job

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13. One specific aspect of choosing efficient programs needs to be mentioned. Since the problem being attacked is teenage unemployment, programs which admit teenagers who are not unemployed are unlikely to be in the efficient set. Thus, efficient programs are likely to require that only unemployed teenagers be eligible. More generally, choice of program mix and choice of eligibility requirements are part of the same decision.

provided would tend to be higher in programs emphasizing future employability than in those aimed at job creation, the decision maker would face a trade-off between number of jobs and training slots.

The distinction between immediate vs. lifetime impacts of teenage unemployment is certainly relevant to the evaluation of this trade-off. Suppose reduction in future employability was the major undesirable impact of teenage unemployment; then a program mix would be chosen which emphasized future employability and training. However, this decision would not completely solve the allocation problem. Given this program mix, decisions would still have to be made concerning the allocation of funds to different geographical areas.

In our previous analysis, when teenage unemployment was viewed solely as a short run problem, the choice of geographical allocation guidelines seemed fairly straightforward. This is so because lack of employment by itself reduces the family income of unemployed teenagers. Thus, the only policy issue is weighting income losses experienced by different income classes. Once the appropriate social welfare function has been specified, funds can be allocated by the relative number of "deserving" families in different regions. In contrast, it is less certain that teenage unemployment by itself causes future unemployability. Thus one must consider how allocation guidelines would vary with different hypotheses about the link between teenage unemployment and future adult employability.

Suppose one accepted the proposition that teenage unemployment per se reduced employability as an adult. Then the teenage unemployment problem would be severest in regions with the most unemployed teenagers. Youth employment and training funds could therefore be allocated solely on the basis of number of unemployed teenagers.

An alternative hypothesis would be that diminished future employability is not caused solely by teenage unemployment, but rather by the interaction of teenage unemployment with other factors such as race and low income family status. In this case, exclusive reliance on unemployment data in making allocation decisions would not be appropriate. For example, suppose that teenage unemployment implied higher adult unemployment only for teenagers in low income families. Then two regions could have equal numbers of unemployed teenagers yet face unequal long-term teenage unemployment problems. This would suggest that youth employment funds be allocated according to the number of unemployed youths in low income families. Similarly, if future employability problems were relatively more severe for unemployed nonwhite youths, allocation of funds on the basis of youth unemployment and race would be appropriate.

Finally, we briefly consider the issues raised by the distinction between cohort-specific versus nonspecific effects. If high levels of teenage unemployment are confined to a particular age cohort, the benefits per dollar spent on information gathering would be less than if teenage unemployment were expected to effect cohorts of youths in the future. However, if relatively high teenage unemployment was not confined to a specific age cohort, funds should be allocated in part with reference to the potential for obtaining policy-relevant information. That is, regardless of program mix, the absence of cohort-specific effects would imply that relatively more funds be devoted to obtaining feedback on each program. Moreover, in each program mix, more funds should be allocated to programs that are policy experiments.

In summary, the differences between the short-term and the long-term effects of teenage unemployment imply some differences in allocation criteria. In both the short- and long-run case, it is useful to separate conceptually allocation decisions pertaining to desired program mix from decisions about the allocation of funds to different regions given program mix. Desired program mix cannot be decided on the basis of simple statistical indicators. When dealing with the short-term effects of teenage unemployment, specification of program mix is dependent on explicit or implicit specification of a social welfare function. Similarly, evidence on the long-term effects of teenage unemployment is needed to choose the desired mix of programs emphasizing job creation and programs emphasizing future employability.

Once the desired program mix has been chosen, scarce program funds must be allocated to different regions. The choice of the appropriate allocation statistic depends on one's views about the nature of the unemployment problem. Finally, one's views about the existence of cohort-specific effects determine the portion of funds allocated to information gathering.

## III. ALLOCATION GUIDELINES IN THE YOUTH EMPLOYMENT DEMONSTRATION PROJECTS ACT

The Act specifies a number of different programs, eligibility requirements for each program, and fund allocation limitations on each program. How consistent is this complex set of regulations with the preferred allocation rules derived above? Our preferred solutions contained separate program mix and fund allocation rules. The Act maintains this distinction; it establishes a centrally determined program mix, at least for major program divisions. However, the similarity between our allocation rules and those of the Act breaks down for less aggregate program mix choices. When teenage unemployment is viewed as a short-run phenomenon, we suggested identical program mixes in each region. However, the Act does not ensure identical program mix by region, since within several broad program categories it allows each area to initiate and design its own programs, subject to some central review. This departs from the recommended solution, thereby implying that marginal benefits per dollar need not be identical across regions.

There are, however, a number of considerations which might justify the Act's procedure. First, suppose that teenage unemployment has long-run effects. In this case, regions might differ in crucial respects, making an identical policy mix across regions less attractive. For example, suppose that long-run effects are most severe for particular demographic groups. If particular groups are not uniformly represented across areas, and different programs work better for some groups than others, different mixes in different areas are desirable. It is not clear, however, that the program selection features of the Act are designed to ensure assignment of the most appropriate programs to each area. A second argument for program diversity across areas involves organizational ability. If areas differ with respect to organizational and administrative skills, local program initiation and design may be more likely to produce programs tailored to local capabilities. Third, if it is not known which programs are the most effective, local initiation may provide a forum for experimentation and development of new program ideas. Finally, it can be argued that CETA (the act which YEDPA amends) considers local autonomy for the prime sponsor as a basic value in itself.[14]

Turning from program mix to funds allocations, we can again ask how close these funds allocations are to our recommendations. If teenage unemployment is viewed solely as a short run phenomenon, our solution suggested that funds be allocated according to the number of unemployed teenagers in poor families. If, on the other hand, teenage unemployment is viewed as having longer run effects, allocation rules would vary with different hypotheses about the link between teenage unemployment and long-term employment problems.

Consider first the Youth Community Conservation Improvement Projects (YCCIP). Seventy-five percent of the available funds go to the states according to the relative number of unemployed persons within each state, subject to minimum percentage requirements for each state and areas such as Guam. The remainder is for discretionary use by the

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[14] We owe this point to a referree. Indeed, the same referree also argues, consistent with our third point, that the development of knowledge through experimentation is a major reason for not specifying program mix in advance.

Secretary of Labor. The entire amount is subject to the requirements that at least 2% be used for native American youth and 2% for youth in migrant and seasonal farmworker families. At least with respect to the 75% of YCCIP funds allocated directly to states, the funding rules do not seem consistent with our guidelines for allocating funds to deal with short run effects of teenage unemployment. The Act allocates funds primarily by number of unemployed persons, whereas the preferred allocation would be by numbers of unemployed teenagers in low income families.

One might ask whether the YCCIP funding rules would be appropriate if teenage unemployment is viewed as having long run impacts. However, the Federal Register discussion of the Act seems to portray YCCIP as a program aimed at short run problems, placing predominant emphasis on the development and provision of jobs.[15] Any training must be directly related to the development of specific skills needed for the job.

Consider next the Youth Employment and Training Program (YETP) component of the Act. The basic allocation guidelines are as follows: (1) 75% of the funds are made available directly to local prime sponsors and 5% to governors. Of this 80%, 37.5% is allocated both across and within states by the number of unemployed persons, 37.5% is allocated by the number of unemployed persons residing in areas of substantial unemployment, and 25% by the number of persons in low income families; (2) 22% of the 75% given to prime sponsors must be used for programs serving in-school youth; (3) at least 2% of the overall funds must be used for native American youth and 2% for youth in seasonal and migrant farmworker families. The remaining funds can be dispersed at the Secretary of Labor's discretion.

If unemployment is perceived to be a short run phenomenon, we argued that funds should be allocated according to the number of poor unemployed teenagers. The Act's rules seem to differ from this benchmark. Seventy-five percent of the funds are allocated according to the number of unemployed persons, not the number of unemployed teenagers. In addition, while 25% of the funds are allocated on the basis of poverty population, poor persons rather than poor teenagers are used as the index.

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[15] Federal Register, Vol. 42, No. 180, September 16, 1977, p. 46730.

The Federal Register states that YETP's purpose is:

> not ... to provide make-work activities but rather to provide youth, especially economically disadvantaged youth with opportunities to learn and earn which will lead to meaningful employment opportunities after they have completed the program.[16]

This seems to imply the YETP is designed to attack presumed long run impacts of teenage unemployment. If so, divergences from recommended allocation rules arise that are similar to those discussed in the short run case. An additional complication is that demographic characteristics other than low income status may need to be taken into account.

Finally, we consider the proposed allocation of funds under the Young Adult Conservation Corps (YACC) and the Youth Incentive Entitlement Pilot Projects (YIEPP). The Labor Department Planning Charter states that YACC will select participants by a "basically random process" with preference given to applicants from areas of "substantial unemployment" (those with a rate of 6.5% or higher).[17] Since YACC relies on total rather than teenage unemployment data to allocate funds, it is subject to the same difficulties discussed above in connection with YICCP and YETP. The final program, YIEPP, combines features of an experimental and demonstration project. Since neither

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16. Ibid., p. 46734.

17. U.S. Department of Labor, Planning Charter for the Youth Employment and Demonstration Projects Act (Washington: Government Printing Office, 1977).

experiments nor demonstration projects are designed to distribute services so as to achieve a desired distribution of benefits, evaluation of the YIEPP guidelines are outside the scope of this paper.

## IV. COMPARISON OF ALTERNATIVE FUNDS ALLOCATIONS

We have argued that several of the allocation guidelines in YEDPA differ from the allocation schemes proposed above. It is possible, however, that differences in concept do not result in significant differences in actual funds allotments. To investigate the empirical impact of using different allocation rules, we compare the distribution of both YCCIP and YETP funds under provisions of the Act with allocations obtained from formulas suggested by our analysis. Table 2 presents the correlation coefficient (r) between the Act's distribution of YCCIP funds and a distribution based on estimated state shares in the number of unemployed poor youth, the recommended allocation if teenage unemployment has only short run effects.[18] We also present the correlation between an approximation of the Act's distribution of YETP funds and distributions implied by two of the formulas consistent with different beliefs about the existence and cause of long run effects.[19] One formula allocates funds on the basis of relative number of unemployed poor youths, the other on the basis of relative numbers of unemployed youths. The correlation coefficient varies between .88 and .97. The significantly higher correlation between the distribution of funds implied by the YETP formula and the formula based on the number of unemployed youths is due to the disproportionate weight given to unemployment in the YETP formula.

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[18] The number of unemployed poor youths by state was estimated as follows. Data from the 1970 census was used because it allowed more extensive disaggregation. First, the number of poor youths by state were obtained by combining numbers of youth aged fourteen to twenty-one in poor families with number of poor unrelated individuals aged fourteen to twenty-four. The slightly different age categories were used because exactly compatible published data were unavailable. The state poverty data were then multiplied by state unemployment ratios (number of unemployed divided by population) for fourteen to twenty-one-year-olds, also obtained from published 1970 census data. While unemployment ratios actually measured for poor youths would be desirable, these were not available from published data. The authors believe that the magnitude of error is unlikely to be significant in estimating the relative impact of different allocation schemes. See U.S. Census, Detailed Population Characteristics, Tables 166 and 207.

Though the correlation falls when the Act's formulas are compared with alternatives that give more weight to poverty, the correlations obtained are still relatively high. However, this does not necessarily mean that the allocations of funds would be similar under alternative formulas. This is readily seen in Table 3, which presents the percentage allocation of YCCIP and YETP funds to regions under alternative allocation schemes. For example, relative to the distribution of funds implied by the Act, formulas based on poverty as well as unemployment would distribute significantly less funds to the Northeast and North Central states and significantly more funds to Southern states. These results may, of course, be subject to qualification due to regional price level differences, existence of state and local minimum wage laws, and other difficulties.[20]

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[19] In order to approximate the distribution of funds under YETP, the following procedure was used. For the 37.5% of relevant YETP funds allocated according to the total number of unemployed in each state, census data on unemployment of individuals sixteen and over was used. For the 37.5% of YETP funds allocated to states according to the number of individuals in areas of substantial unemployment, the actual 1973 measures used in making CETA allocations for Fiscal 1974 were employed. These were obtained in unpublished form from the Employment and Training Administration, U.S. Department of Labor. The 1973 data were used since the actual CETA procedure for specifying areas of substantial unemployment is complex and nonreproducable , so that determining what such areas would have been in 1970 if CETA had existed is impossible. The year 1973 was used because that was the first year after 1970 that such figures were calculated. For the 25% allocated according to the number of persons in families below a particular low income line, we used census data on the number of family members below 125% of the poverty line. While this is conceptually appealing, and consistent with the language in the Act, it does differ from the actual administrative procedure, which fails to allow for different income cutoffs by family size! We believe that using the administrative procedure would have resulted in larger empirical divergences between the recommended allocation rules and those in the Act than those reported in the text. Census data used in the YETP calculation are from U.S. Census, Detailed Population Characteristics, Tables 168 and 207.

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[20] A referree suggested these qualifications. If there are price disparities across regions, dollar allocations may need to be adjusted for purchasing power differences. Such an adjustment would probably result in a decline in the percentage allocation going to the South, and a rise in the percentage allocation going to the Northeast. Unfortunately, the art of measuring such regional differentials is still quite primitive, and there is reason to believe that frequently used measures overstate differences (see, for example, Mark Sherwood "Family Budgets and Geographical Differences in Price Levels," Monthly Labor Review, vol.98, April 1975, pp. 8-15 and Timothy Smeeding, "Measuring the Economic Welfare of Low Income Households, and the Antipoverty Effectiveness of Cash and Non-Cash Transfer Programs," unpublished Ph.D. dissertation University of Wisconsin, 1975). Moreover, there are a variety of existing federal transfer programs which do not make such corrections, and there exists considerable controversy concerning the appropriateness of such corrections. State and local minimum wage laws raise two issues. First, suppose that a particular state has a minimum wage law that raises teenage unemployment (either because the state minimum wage is higher than the federal, or because it covers some jobs the federal minimum wage does not cover). That state is "rewarded" for its high minimum wage by getting more federal funds under our proposed allocation formula. Even if one wanted to "correct" for this result, it is not obvious how one would do so. Second, if the state minimum wage law requires higher pay for some teenage job-creation programs, the same amount of dollar funds will not create as many job slots in that state. This problem bears a resemblance to the regional price variation problem. A final difficulty concerns teenage unemployment among illegal aliens and other "undocumented workers." To the extent that the measured unemployment rate fails to include these individuals, areas with heavy concentrations of such individuals may be getting smaller allocations than their "true" unemployment situation warrants. Of course, one's views on this issue depend on one's beliefs about whether government programs should be concerned with unemployment of illegal aliens along with "native" unemployment.

Our calculations indicate that different series may produce different funds allocations by state. Our analysis implied that funds be allocated on the basis of detailed characteristics of unemployed teenagers readily available only in the decennial census. Since funding decisions must be made yearly, however, ways of using annual data to generate allocation rules would have to be developed. We now consider how census information could be used to improve the quality of these rules.

There are many series--various unemployment rates, poverty rates, and so forth--which have some relevance for allocation decisions. How can these series be combined to produce information as close as possible to census data? Suppose that teenage unemployment is viewed as a short run phenomenon, so that the desired allocation statistic is the number of unemployed poor teenagers. Unfortunately, this statistic is not available annually. Suppose, however, that separate series on unemployment and poverty by state were available. These two statistical series could be combined to form a proxy for the desired statistic through regression analysis. Cross-section data on the number of unemployed poor teenagers obtainable from the 1970 Census could be regressed on 1970 observations of the annually available series on unemployment and poverty. The estimated regression could then be used in subsequent years to estimate the number of unemployed poor teenagers from the annually available data.

Two comments on this procedure are relevant. First, the actual choice of unemployment or poverty series would be based on statistical criteria, such as goodness of fit. Second, the attractiveness of this procedure depends on the stability of the estimated relationship through time. Third, for any choice of series this procedure has the attraction that those series would be combined on the basis of their relationship to the desired allocation statistic, rather than through ad hoc weights.

## TABLE 2

## COEFFICIENT OF CORRELATION (r) BETWEEN THE DISTRIBUTION OF YCCIP AND YETP FUNDS UNDER ALTERNATIVE ALLOCATION RULES

| | |
|---|---|
| r Between the Act's distribution of YCCIP funds and distribution of funds according to state shares of unemployed poor youths. | .90<br>(.83-.94)* |
| r Between the Act's distribution of YETP funds and distribution of funds according to state shares of unemployed poor youths. | .88<br>(.80-.93) |
| r Between the Act's distribution of YETP funds and distribution of funds according to state shares of unemployed youths. | .97<br>(.95-.99) |

*Numbers in parentheses are the 95 percent confidence intervals for the correlation coefficients.

## TABLE 3

### PERCENTAGE ALLOCATION OF YCCIP AND YETP FUNDS TO GEOGRAPHICAL REGIONS UNDER ALTERNATIVE ALLOCATION FORMULAS

| Region | Percent Allocation Under Act's YCCIP Formula | Percent Allocation Under Act's YETP Formula | Percent Allocation Based on State Shares of Unemployed Poor Youths | Percent Allocation Based on State Shares of Unemployed Youths |
|---|---|---|---|---|
| Northeast | 22.2 | 24.8 | 13.1 | 18.2 |
| North Central | 27.1 | 24.6 | 22.4 | 30.0 |
| South | 26.2 | 27.2 | 42.4 | 28.5 |
| West | 24.6 | 23.4 | 22.1 | 23.3 |

## V. SUMMARY AND IMPLICATIONS

Several results have emerged from our analysis. First, it is useful to separate conceptually program mix decisions from funds allocation decisions given program mix. Second, the appropriate allocation scheme depends on whether the bad effects of teenage unemployment are believed to be temporary or permanent. Third, different allocation statistics can result in divergent distribution of funds. Fourth, even though annual data on desired allocation statistics may not be available, it may be possible to construct reasonably good proxies from available data. Specifically, we have suggested a potentially useful method for combining series based on regression techniques.

The Act already specifies the way in which a majority of funds must be allocated. However, there remains a sizable block of funds for which specific allocation rules have not been mandated. Two alternative approaches for allocating these funds may be identified. One procedure would be to decide de novo on a desired program mix and then allocate funds given program mix using the general guidelines set forth above. An alternative approach based on "second best" considerations would be to: (1) calculate a desired overall distribution, (2) determine the divergence between the desired distribution and that resulting from allocation formulas specified in the Act, and (3) distribute discretionary funds so as to offset this divergence.

# LOWERING YOUTH UNEMPLOYMENT: HOW MUCH AND AT WHAT COST?

By: Paul O. Flaim

and

Paul M. Ryscavage

## ABSTRACT*

This paper examines the issue of how low the unemployment rate for youths might be brought, and points out that for the entire sixteen to twenty-four group the rate has never averaged less than 8% for any year since the Korean War. In addition, we found that in some recent "full-employment" estimates the youth jobless rates have been pegged even higher.

With regard to the wages that would have to be paid to youths placed in job programs, we presented several distributions, all of which show a large dispersion. Many youths, particularly in the sixteen to nineteen group, are apparently willing to work for less than the federal minimum. At the same time, many others, particularly those twenty to twenty-four, are generally earning much more--and expect to earn much more--than the minimum.

The averages from these earnings distributions were used along with the Federal minimum, to compute some illustrative estimates of what it might have cost in 1976 to place enough unemployed youths in jobs so as to lower their incidence of unemployment to the levels of 1969 and, alternatively, to those of 1973. The lowest cost estimates were obtained by using the federal minimum and the highest by applying the "prevailing earnings" of employed youths.

## INTRODUCTION

The purpose of this paper was to make some estimates of what it might cost to put the unemployed youth to work given various assumptions as to how low their unemployment rate could be brought and as to how much they would have to be paid in order to get them to take jobs. In making these estimates, we had to examine various questions, such as:

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* The estimates and opinions which are presented in this paper do not necessarily reflect the views or have the endorsement of the Bureau of Labor Statistics.

(1.) Would it be possible--or even desirable--to find or create jobs for all of the unemployed youth?

(2.) If not, how many could be realistically placed into job programs?

(3.) What type of jobs--full time, part time, minimum wage, or otherwise--would these have to be?

(4.) Might the availability of such jobs bring additional youth into the labor market and thus vitiate the efforts to lower the group's unemployment?

(5.) If so, could it still be determined objectively whether the job programs have been a success, a partial success or a failure?

In examining these difficult questions, we had access to some interesting new data on unemployed youths as well as to the traditional historical series. And while the new data, which will be discussed below, do not go very far toward answering the questions posed above, they should shed some useful light on these issues.

## HOW LOW CAN YOUTH UNEMPLOYMENT BE BROUGHT?

Most people would readily agree that youth unemployment cannot be eliminated in its entirety; that there is some minimal, "natural," or "frictional" rate of unemployment for every population group; and that for youth--the group most lacking in skills and experience--this rate is higher than for any other group. The problem is that such a rate is not readily available. Even if sought through empirical research and a most detailed look at the data, such a rate would prove to be very elusive. At best, research might yield a range within which such a rate might be located.

While the search for such a rate is outside the scope of this paper, history does provide some guidelines as to the lowest levels attained by the unemployment rate for youth under various economic conditions. As shown in Table 1, for example, the rate for youth sixteen to nineteen has never been below 10% since the end of the Korean War. And this period includes some years--such as 1955 and 1956, and then again 1969--when the economy is generally regarded as having operated at or possibly even above its full-employment potential.

As for the rate for youths twenty to twenty-four years of age, it only dipped below 6% in four years over the 1954-1977 period. This was during the Vietnam war when much of the potential labor force in this age group was being syphoned off into the Armed Forces (and when quite a few of these youths prolonged their schooling or skipped the country in order to avoid the draft).

If we look at the jobless rate for the entire sixteen to twenty-four group, we see that it has never been below 8% since the mid-1950's. Over this period there were seven years in which it dropped to the 8-9% range, but somehow it seemed unable to move below this range. And here we might note that if youths spent one-twelfth of their labor force time in the job search process--that is, one month a year if they are in the labor force all year--unemployment among their group would average 8.25%, very much in line with the "lows" attained by their jobless rate over the past two decades.

And there are those who would peg the unemployment goals for youths even higher for the current years. They would do so generally on the basis that the youth's proportion of the labor force has increased considerably since the mid-1960s and that the resulting "crowding effect" has gradually raised what one might call the "natural" unemployment rate for youths. Thus, Michael L. Wachter, in computing a "non-inflationary unemployment rate" for the nation, which he estimated to be 5.5% for 1975, pegged the youth rates for the same year at 15.7% for males sixteen to nineteen, 8.2% for males twenty to twenty-four, 16.9% for females sixteen to nineteen and 9% for females twenty to twenty-

TABLE 1.

UNEMPLOYMENT RATES FOR YOUTHS 16 TO 24 YEARS OF AGE: ANNUAL AVERAGES, 1954-1977

| Year | Total, 16 to 24 years | 16 to 19 years | | | 20 to 24 years |
|---|---|---|---|---|---|
| | | Total, 16 to 19 years | 16 and 17 | 18 and 19 | |
| 1954---------------- | 10.6 | 12.6 | 13.5 | 12.0 | 9.2 |
| 1955---------------- | 8.7 | 11.0 | 12.3 | 10.0 | 7.0 |
| 1956---------------- | 8.5 | 11.1 | 12.3 | 10.2 | 6.6 |
| 1957---------------- | 9.0 | 11.6 | 12.5 | 10.9 | 7.1 |
| 1958---------------- | 13.1 | 15.9 | 16.4 | 15.5 | 11.2 |
| 1959---------------- | 11.0 | 14.6 | 15.3 | 14.0 | 8.5 |
| 1960---------------- | 11.2 | 14.7 | 15.5 | 14.1 | 8.7 |
| 1961---------------- | 13.0 | 16.8 | 18.3 | 15.8 | 10.4 |
| 1962---------------- | 11.3 | 14.6 | 16.2 | 13.6 | 9.0 |
| 1963---------------- | 12.2 | 17.2 | 19.3 | 15.6 | 8.8 |
| 1964---------------- | 11.5 | 16.2 | 17.8 | 14.9 | 8.3 |
| 1965---------------- | 10.1 | 14.8 | 16.5 | 13.5 | 6.7 |
| 1966---------------- | 8.6 | 12.7 | 14.8 | 11.3 | 5.3 |
| 1967---------------- | 8.7 | 12.9 | 14.7 | 11.6 | 5.7 |
| 1968---------------- | 8.7 | 12.7 | 14.7 | 11.2 | 5.8 |
| 1969---------------- | 8.4 | 12.2 | 14.5 | 10.5 | 5.7 |
| 1970---------------- | 11.0 | 15.3 | 17.1 | 13.8 | 8.2 |
| 1971---------------- | 12.7 | 15.9 | 18.7 | 15.5 | 9.9 |
| 1972---------------- | 12.1 | 16.2 | 18.5 | 14.6 | 9.3 |
| 1973---------------- | 10.5 | 14.5 | 17.3 | 12.4 | 7.8 |
| 1974---------------- | 11.8 | 16.0 | 18.4 | 14.2 | 9.0 |
| 1975---------------- | 16.1 | 19.9 | 21.4 | 18.9 | 13.6 |
| 1976---------------- | 14.7 | 19.0 | 21.1 | 17.4 | 12.0 |
| 1977---------------- | 13.6 | 17.7 | 19.9 | 16.2 | 10.9 |

four.[1] And Peter Clark, in arriving at a 5.1% "high employment benchmark unemployment rate" for 1977, pegged the youth rates at the following levels: 14.8% for males sixteen to nineteen, 7.1% for males twenty to twenty-four, 17.7% for women sixteen to nineteen, and 9.1% for women twenty to twenty-four.[2]

As shown below, while Wachter's and Clark's unemployment "goals" for youths in 1975 and 1977 are substantially lower than the actual rates experienced by youths in recent years, they are still much higher than the rates which had been achieved by youths in 1969. Wachter's estimates, in particular, are even somewhat higher than the rates experienced by youths in 1973, when the overall unemployment rate for the nation (4.9%) was at about the halfway point between the 1969 rate (3.5%) and the current rate (6.4% as of December 1977). Only as the youth groups diminish in size relative to the other population groups--a development that is now in its initial stages and which will continue through the 1980s, given the sustained decline in the birth rate during the 1960s and early 1970s--would the jobless rates for youths, as estimated by Wachter and Clark, be expected to decline much below their 1973 levels.

Nevertheless, while cognizant of the Wachter and Clark estimates shown above, we still decided to use the actual numbers for 1969 and 1973 as the benchmarks for our calculations of the size and cost of the youth job deficit. In essence, what we did was to calculate what it would cost to lower the unemployment rates for youths from the 1976 levels to the 1969 levels and, alternatively, what it would cost to span the much narrower differences between the 1976 rates for youths and those for 1973. In so doing, we are endorsing neither the 1969 nor the 1973 rates as the most desirable goals. We make no such judgments, although we are aware that many will regard the 1969 rates as too low and highly inflationary, while many others might regard the 1973 rates as still far too high. Yet others might look at the data for specific groups of youth, such as black teenagers, and argue that even the 1969 rate for this group (24%) was excessively high.

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[1] Michael L. Wachter, "The Demographic Impact on Unemployment: Past Experience and the Outlook for the Future," in *Demographic Trends and Full Employment*, Special Report Number 12 of the National Commission for Manpower Policy, Washington, D.C., 1976, pp. 27-99.

[2] Peter K. Clark, "Potential GNP in the United States: 1948-1980" in *U.S. Productive Capacity: Estimating the Utilization Gap*," Center for the Study of American Business, Washington University, St. Louis, Mo., December 1977, p. 37.

It is simply that, as we shall see below, the method we chose for making our alternative sets of cost estimates dictated the use of data on the distribution of unemployment for given years. And the juxtaposition of data for 1969 and 1973 with those for 1976 yields estimates which, while differing sufficiently from each other, are within the bounds of historical reality.

## WHAT JOBS AND WHAT WAGES FOR YOUTHS?

Even after we had chosen a set of unemployment rates, actual or hypothetical, we still had but one of the three basic factors needed for our calculations. Two other important factors are also needed: the number of hours and weeks of work to be provided to the youths who would have to be placed in jobs in order to reach the desired rates of unemployment and the wages that would have to be paid to them in order to attract them to these jobs.

While the decisions with regard to these issues would, in the final analysis, be made by policy makers, they must be made in light of the desires and aspirations of the youths they would effect. Although these aspirations cannot be pinpointed with precision, new data collected in recent years, combined with data which have long been available, shed considerable light both on the expectations of unemployed youths as well as on the work patterns and earnings of the employed ones.

In terms of the number hours of work that might be possibly provided to unemployed youths, one can look at the very detailed data that have been available for many years on the hours actually worked by employed youth. Also available are data on the unemployed in terms of whether they want a full-time or a part-time job, or whether their last job was of full-time or part-time nature.

In terms of the hourly wages that would be required to entice youths to take jobs, one could, of course, assume that the minimum wage could be applied across the board. However, while we have used the Federal minimum to construct some of our alternative estimates, we have found that there is a great diversity among youths both in terms of the actual wages earned by those who are employed as well as in terms of the wage aspirations of those who are locking for jobs. This diversity is amply illustrated in Tables 2, 3, and 4. Tables 2 and 3, which are based on special data collected from a sample of unemployed persons in May 1976, show, respectively, the hourly rates of pay for the last job previously held by unemployed youths and the so-called "reservation wages" in terms of the jobs they were seeking at the time. Table 4, based on data collected each year, shows what employed youths were earning in May 1976.

As shown, while some of the overall averages for these tables (particularly the medians) do not differ widely from the federal minimum wage--which was $2.30 an hour in May 1976--the actual distributions show a considerable dispersion around the averages. For example, the data in Table 2 show that many of the unemployed youths with previous work experience had last worked for wages far below the minimum while many others had apparently earned much more than the minimum. And even when it came to the so-called "reservation wage," or the lowest wages that youths were willing to settle for in their next jobs (Table 3), we again see a large dispersion around the averages. Apparently, many youths were willing to accept wages considerably lower than the minimum (which is still legal in many situations) while many others had much higher expectations. And the earnings distribution for

## TABLE 2.

## REPORTED HOURLY EARNINGS FOR LAST JOBS HELD BY YOUTHS 16 TO 24 YEARS OLD WHO WERE UNEMPLOYED IN MAY 1976

| Age, sex, and race | Total (thousands) | Under $2.00 | $2.00 to $2.29 | $2.30 | $2.31 to $2.99 | $3.00 to $3.99 | $4.00 to $4.99 | $5.00 and over | Mean | Median |
|---|---|---|---|---|---|---|---|---|---|---|
| Total, 16 to 24 years------ | 1,001 | 97 | 356 | 87 | 198 | 127 | 72 | 64 | $2.75 | $2.30 |
| 16 to 19 years------ | 481 | 62 | 217 | 49 | 95 | 32 | 17 | 9 | 2.37 | 2.20 |
| 20 to 24 years------ | 520 | 34 | 139 | 38 | 104 | 95 | 54 | 55 | 3.10 | 2.50 |
| Males, 16 to 24 years------ | 542 | 32 | 137 | 51 | 110 | 94 | 60 | 59 | 3.11 | 2.50 |
| 16 to 19 years------ | 269 | 23 | 105 | 30 | 60 | 27 | 14 | 9 | 2.54 | 2.30 |
| 20 to 24 years------ | 273 | 8 | 32 | 21 | 49 | 67 | 46 | 50 | 3.67 | 3.25 |
| Females, 16 to 24 years---- | 459 | 65 | 219 | 36 | 89 | 33 | 12 | 5 | 2.32 | 2.20 |
| 16 to 19 years---- | 212 | 39 | 112 | 19 | 34 | 5 | 3 | -- | 2.16 | 2.10 |
| 20 to 24 years---- | 247 | 26 | 107 | 17 | 54 | 28 | 9 | 5 | 2.46 | 2.25 |
| White, 16 to 24 years------ | 817 | 76 | 285 | 77 | 156 | 109 | 60 | 54 | 2.77 | 2.30 |
| Black, 16 to 24 years------ | 177 | 20 | 71 | 10 | 39 | 17 | 12 | 8 | 2.60 | 2.25 |

## TABLE 3.

## LOWEST ACCEPTABLE HOURLY EARNINGS OF YOUTHS 16 TO 24 YEARS OLD WHO WERE UNEMPLOYED IN MAY 1976

| Age, sex, and race | Total (thousands) | Under $2.00 | $2.00 to $2.29 | $2.30 | $2.31 to $2.99 | $3.00 to $3.99 | $4.00 to $4.99 | $5.00 and over | Mean | Median |
|---|---|---|---|---|---|---|---|---|---|---|
| Total, 16 to 24 years------ | 1,494 | 68 | 387 | 266 | 334 | 279 | 72 | 88 | $2.75 | $2.35 |
| 16 to 19 years------ | 816 | 46 | 279 | 173 | 184 | 96 | 23 | 15 | 2.47 | 2.30 |
| 20 to 24 years------ | 678 | 22 | 107 | 93 | 151 | 183 | 49 | 73 | 3.10 | 2.50 |
| Males, 16 to 24 years----- | 784 | 30 | 158 | 104 | 161 | 192 | 59 | 80 | 3.05 | 2.50 |
| 16 to 19 years----- | 423 | 25 | 130 | 73 | 94 | 66 | 20 | 15 | 2.61 | 2.30 |
| 20 to 24 years----- | 362 | 5 | 27 | 32 | 67 | 126 | 39 | 66 | 3.57 | 3.00 |
| Females, 16 to 24 years--- | 710 | 38 | 229 | 161 | 173 | 88 | 13 | 8 | 2.42 | 2.30 |
| 16 to 19 years--- | 394 | 21 | 149 | 100 | 91 | 31 | 3 | -- | 2.32 | 2.30 |
| 20 to 24 years--- | 316 | 18 | 80 | 61 | 83 | 57 | 10 | 7 | 2.56 | 2.30 |
| White, 16 to 24 years------ | 1,175 | 57 | 301 | 218 | 245 | 220 | 66 | 69 | 2.76 | 2.35 |
| Black, 16 to 24 years------ | 302 | 11 | 79 | 48 | 87 | 53 | 6 | 19 | 2.74 | 2.50 |

TABLE 4.

ACTUAL HOURLY EARNINGS OF EMPLOYED YOUTHS 16 TO 24 YEARS OLD WHO WERE PAID BY THE HOUR, MAY 1976

| Age, sex, and race | Total reporting (thousands) | Under $2.00 | $2.00 to $2.24 | $2.25 to $2.49 | $2.50 to $2.99 | $3.00 to $3.99 | $4.00 to $4.99 | $5.00 to $5.99 | $6.00 to $7.99 | $8.00 and over | Mean | Median |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Total, 16 to 19 years----------- | 4,770 | 583 | 1,016 | 1,332 | 898 | 678 | 145 | 70 | 41 | 8 | $2.52 | $2.40 |
| 20 to 24 years----------- | 6,357 | 193 | 371 | 800 | 1,070 | 1,866 | 1,013 | 566 | 403 | 75 | 3.60 | 3.33 |
| Males, 16 to 19 years----------- | 2,534 | 174 | 531 | 622 | 508 | 467 | 131 | 61 | 36 | 5 | 2.70 | 2.48 |
| 20 to 24 years----------- | 3,671 | 42 | 123 | 284 | 520 | 1,086 | 736 | 453 | 358 | 68 | 3.96 | 3.78 |
| Females, 16 to 19 years--------- | 2,237 | 409 | 486 | 709 | 390 | 211 | 14 | 10 | 5 | 3 | 2.32 | 2.33 |
| 20 to 24 years--------- | 2,685 | 151 | 247 | 515 | 550 | 781 | 278 | 113 | 45 | 7 | 3.11 | 2.89 |
| White, 16 to 19 years----------- | 4,450 | 561 | 919 | 1,240 | 854 | 625 | 139 | 65 | 40 | 8 | 2.53 | 2.40 |
| 20 to 24 years----------- | 5,570 | 170 | 312 | 645 | 939 | 1,667 | 897 | 518 | 367 | 56 | 3.37 | 3.60 |
| Black, 16 to 19 years----------- | 321 | 21 | 98 | 92 | 44 | 53 | 5 | 6 | 1 | -- | 2.50 | 2.36 |
| 20 to 24 years----------- | 787 | 22 | 59 | 155 | 132 | 199 | 117 | 48 | 36 | 19 | 3.65 | 3.09 |

youths who were actually working in May (Table 4) also shows a wide dispersion. The earnings averages for these employed youths were also generally higher than those reported by unemployed youths either in terms of previous or future jobs.

What all of these tables also show, and not unexpectedly, is that youths twenty to twenty-four earn much more--and expect to earn much more--than those sixteen to nineteen years of age. For the older group, the earnings averages, both in terms of actual earnings on past or current jobs as well as in terms of the reservation wages, are considerably higher than the $2.30 minimum wage which prevailed when these data were obtained. Only about one-third of these youths, compared with a little over one-half of those sixteen to nineteen, appear to have been willing to take jobs paying only the Federal minimum.

As shown, the wage aspirations of unemployed black youths did not differ that much from those of unemployed white youths. The medians for blacks were either the same or slightly lower than those for whites. In terms of the means, the pattern was not as consistent, but it must be remembered that here we are dealing with relatively small distributions, where the means can be affected by only a few unusually high values. (Thus, the fact that the mean reservation wages for black teenagers turned out to be slightly higher than that for white teenagers should not be given too much weight.)

The earnings data for youths who are actually employed are perhaps the most relevant in determining what it would cost to put their unemployed counterparts to work. The problem which arises in using these data is that unemployed workers as a group do not have the same characteristics in terms of skills and education as do those who are employed and could not, therefore, be expected to command the same wages. It is only after account is taken of the major differences in personal characteristics between employed and unemployed youth that the earnings data for the former can be used to figure out what it would cost to employ the latter.

Earnings averages which do take into account such differences, both for the unemployed youth as well as for older unemployed persons, were recently developed by applying regression analysis to the data on usual weekly earnings that are collected each May. These regression-derived estimates show what the weekly earnings of the unemployed would have been, assuming that they would have received the same rates of pay as did employed persons who had the same characteristics in terms of age, sex, race, education, occupation, geographic residence, and full- or part-time status.[3] While the distributions of such hypothetical earnings were not tabulated, averages were computed for various youth groups as well as for older workers.

By using this "peer group" methodology, it was estimated that the average (mean) amount of earnings lost because of unemployment in May 1976 was about \$110 a week for males sixteen to nineteen, \$85 a week for females sixteen to nineteen, \$158 a week for men twenty to twenty-four, and \$135 a week for women twenty to twenty-four. These "peer group" estimates, which might be regarded as representing the "potential" prevailing earnings of unemployed youths--provided, of course that they had access to the same mixture of jobs as were then being held by employed youths and that the wage structure would remain the same--have been used to make one of the alternative sets of calculations of the total cost of lowering youth unemployment. Different calculations were made on the basis of the reservation wages and on the basis of the federal minimum.

## ESTIMATING THE YOUTH WAGE BILL

After settling on two sets of unemployment benchmarks for youths, that is, the incidence of unemployment encountered among them in 1969 and in 1973, the general procedure used in estimating the cost of reaching these goals was as follows: First we estimated, on the basis of the three wage assumptions, what it would have actually cost in terms of total wages to put all of the unemployed youth to work in 1976--that is, to bring youth unemployment down to zero. We then estimated what it would have cost if the incidence of youth unemployment and its distribution in 1976 were consistent with the situation in 1969 and 1973. We then computed the differences between the actual total wage cost in 1976 and the two hypothetical costs. These differences represent our estimates of the wage costs associated with job programs designed to lower the incidence of youth unemployment from its levels of 1976 to those of 1969 and 1973, when the economy was operating much closer to (or perhaps even slightly above) its full potential.

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[3] Paul M. Ryscavage and Curtis L. Gilroy, "Earnings Foregone by the Unemployed," in Proceedings of the Business and Economics Statistics Section, American Statistical Association, 1977, pp. 654-9.

The job deficit. The statistics that were used to determine how many youths would have to have been placed in jobs in 1976--and for how long--in order to achieve these goals were those from the annual "work experience" survey. These data show the total number of youths who encounter one or more spells of unemployment during the year, as well as the total length of their unemployment.[4] For 1969, a year when the unemployment rate for the nation averaged only 3.5% and the rate for youths sixteen to twenty-four averaged 8.4%, these "work experience" numbers show that a total of 4.8 million youths encountered one or more spells of unemployment during the year (See Table 5). These 4.8 million youths accounted for 16.4% of their civilian noninstitutional population.

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[4] The total number of different persons who encounter some unemployment during any given year is obtained through special questions which are asked each March and which relate to employment and the number and duration of possible spells of unemployment encountered during the year. In a loose way, these "work experience" numbers may be regarded as an aggregation of the "flows" into and out of unemployment during a given year, whereas both the monthly and the annual average numbers might be regarded as reflecting the "stock" or the average of various measurements of the stock taken during the year. In theory, these two sets of numbers should be consistent with each other in terms of yielding the same number of person-weeks and person-hours of unemployment for a given year. In practice, however, these two sets of data have been found to yield somewhat different aggregates. For youths, in particular, the work experience data have been found to underestimate unemployment relative to the estimates obtained from the monthly survey. For a detailed discussion of the different estimates of unemployment that emerge from the "stock" and from the "flow" measurements, see R.D. Morgenstern and N.S. Barret, "The Retrospective Bias in Unemployment Reporting by Sex, Race, and Age," Journal of the American Statistical Association, June 1974, pp. 335-57.

During 1976, the number of youths who encountered at least one spell of unemployment was 8.5 million, or 24.1% of the youth's civilian noninstitutional population. In order to quantify properly the difference between the number of youths who would have been unemployed in 1976 had the 1969 condition prevailed, but still allow for the growth in population between these two years, the proportion of youths who encountered unemployment in 1969 was applied to the 1976 youth population. The difference between the actual numbers for 1976 and the hypothetical ones based on 1969 proportions represented the number of youths that would have to be placed in jobs of various duration in 1976 in order to bring their rates of unemployment to 1969 levels (Table 5). The same procedure was then followed to calculate the number of youth that would have required jobs in 1976 in order to lower their incidence of unemployment to the levels encountered in 1973. According to these procedures, 2.8 million youths would have needed jobs of various duration in 1976 in order to bring their group's joblessness to 1969 levels. About 1.9 million would have needed jobs of various duration in order to reduce their group's jobless rates to 1969 levels.

The next step in the estimating procedure was to calculate for how long each of these youths would have to be employed in terms of weeks and whether the jobs to be provided them would have to be of a full- or part-time nature.[5] In order to do this, the percent distributions of

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[5] It was assumed that youths whose longest job during the year was of full-time nature would be looking for full-time jobs, while those whose longest job was of part-time nature would have wanted part-time jobs. The small group who had looked for work but did not have a job during the year was divided into two equal parts, with half assumed to have been looking for full-time work and half assumed to have wanted part-time jobs.

TABLE 5.

UNEMPLOYMENT RATES AND THE EXTENT OF UNEMPLOYMENT AMONG YOUTH, 1969, 1973, AND 1976

| Selected characteristics by age | Actual data for: | | | Hypothetical data for 1976 if unemployment were distributed as in: | |
|---|---|---|---|---|---|
| | 1969 | 1973 | 1976 | 1969 | 1973 |
| nnual average unemployment rate, all persons 16 years and over------------------ | 3.5 | 4.9 | 7.7 | -- | -- |
| Youth unemployment rate, 16 to 24 (percent)- | 8.4 | 10.5 | 14.7 | 8.4 | 10.5 |
| Teenage unemployment rate, 16 to 19------- (percent) | 12.2 | 14.5 | 19.0 | 12.2 | 14.5 |
| Young workers' unemployment rate, 20 to 24 (percent)------ | 5.7 | 7.8 | 12.0 | 5.7 | 7.8 |
| Civilian noninstitutional population 1/ 16 to 24 years of age (000)---- | 29,382 | 33,531 | 35,405 | 35,405 | 35,405 |
| Percent of youth population with some unemployment during year------------------ | 16.4 | 18.8 | 24.1 | 16.4 | 18.8 |
| Persons 16 to 24 years of age with some unemployment during year (000)------------- | 4,827 | 6,306 | 8,524 | 5,806 | 6,658 |

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1/ These are the noninstitutional population counts for the month of March following the year of reference.

youth unemployment for 1969 and 1973 (in terms of age, sex, full or part-time status, and length of unemployment) were applied to the hypothetical levels of unemployment for 1976. The resulting distributions of unemployment were then costed out according to several different assumptions about the wages to be paid.

It should be noted that an important assumption that is implicit in this procedure is that short-duration unemployment can be dealt with through job creation as effectively as unemployment of longer duration. Yet it is doubtful that much could be realistically done through job programs to deal with unemployment of short duration--say, that lasting four weeks or less. And, as can be seen from Table 6, a very large proportion of the youths who encountered some unemployment in 1976 were jobless for only very short periods.

Another critical assumption that was made is that, if placed in job programs, these youths would have remained there only for as long as they would have been unemployed if they had not taken these placements. In other words, no allowance was made in making these estimates for any possible increase in the labor supply of youths in response to these programs. This is, admittedly, another very questionable assumption.

Youth's wages and earnings. In order to produce a range of estimates associated with the youth job deficit we used the minimum wage, the lowest acceptable wages and the prevailing earnings of youth in our calculations.

In so doing, we first assumed that the minimum wage of $2.30 would have applied across the board to all unemployed youth, regardless of age, sex, or type of job last held. On the other hand, the calculation based on the lowest acceptable wages and the peer group earnings (or "prevailing earings") made use of different averages (means) for specific age-sex groups. Specifically, four different lowest acceptable wage levels (by age and sex) were applied to the lost hours of unemployment accumulated by youths, while eight different peer-group earnings levels (by age, sex, full-time and part-time status) were applied to

## TABLE 6.

## DISTRIBUTION OF YOUTHS WITH SOME UNEMPLOYMENT IN 1976 BY NUMBER OF WEEKS UNEMPLOYED

| Age, sex, race, and work status during year | Total with some unemployment (thousands) | Percent distribution by total weeks of unemployment | | | | | | Percent with: | |
|---|---|---|---|---|---|---|---|---|---|
| | | Total | 1 to 4 weeks | 5 to 10 weeks | 11 to 14 weeks | 15 to 26 weeks | 27 weeks and over | 2 spells of unemployment | 3 or more spells |
| WORKED AT LEAST PART OF YEAR | | | | | | | | | |
| Total: 16 and 17 years | 889 | 100.0 | 39.6 | 19.5 | 10.4 | 16.8 | 13.7 | 17.3 | 16.0 |
| 18 and 19 years | 1,819 | 100.0 | 31.9 | 21.2 | 11.4 | 17.2 | 18.3 | 21.2 | 15.3 |
| 20 to 24 years | 4,540 | 100.0 | 28.1 | 20.2 | 12.0 | 22.6 | 17.1 | 18.2 | 15.9 |
| Male: 16 and 17 years | 463 | 100.0 | 34.0 | 19.2 | 10.2 | 18.0 | 18.5 | 20.7 | 17.4 |
| 18 and 19 years | 966 | 100.0 | 28.4 | 19.9 | 12.3 | 17.3 | 22.1 | 23.2 | 17.2 |
| 20 to 24 years | 2,707 | 100.0 | 23.7 | 20.6 | 12.4 | 24.3 | 19.0 | 20.3 | 17.7 |
| White: 16 to 19 years | 265 | 100.0 | 31.5 | 19.6 | 12.1 | 17.6 | 19.2 | 22.1 | 17.1 |
| 20 to 24 years | 137 | 100.0 | 24.9 | 21.6 | 12.4 | 23.8 | 17.3 | 20.2 | 16.8 |
| Black: 16 to 19 years | 168 | 100.0 | 21.7 | 18.4 | 7.7 | 17.5 | 34.7 | 24.8 | 19.3 |
| 20 to 24 years | 336 | 100.0 | 16.8 | 13.8 | 12.0 | 27.8 | 29.6 | 22.6 | 22.5 |
| LOOKED FOR JOB BUT DID NOT WORK DURING YEAR | | | | | | | | | |
| Total: 16 and 17 years | 414 | 100.0 | 41.3 | 20.3 | 12.1 | 13.8 | 12.6 | N.A. | N.A. |
| 18 to 24 years | 862 | 100.0 | 32.7 | 17.2 | 8.9 | 11.8 | 29.4 | N.A. | N.A. |
| Male: 16 and 17 years | 229 | 100.0 | 37.3 | 21.8 | 11.4 | 14.2 | 15.2 | N.A. | N.A. |
| 18 to 24 years | 328 | 100.0 | 20.6 | 13.4 | 11.6 | 10.6 | 43.8 | N.A. | N.A. |
| Female: 16 and 17 years | 185 | 100.0 | 46.5 | 18.4 | 13.0 | 13.0 | 9.2 | N.A. | N.A. |
| 18 to 24 years | 534 | 100.0 | 40.1 | 19.5 | 7.3 | 12.6 | 20.5 | N.A. | N.A. |

N.A.=NOT AVAILABLE

the weeks of unemployment accumulated during the year by the various groups of youths. In the last two cases, we assumed that the wage and earnings levels which were obtained in May were representative of the average for the entire year. (The data relating to the earnings on the last jobs held by the unemployed were not used as the basis for any calculations both because they are, by definition, the most dated, as well as because the averages from these series did not differ that much from those based on the reservation wages. For a fuller description of the sources of the data used and of the assumptions made, see the Technical Note beginning on page 394.)

The results. Table 7 shows what it would have cost in 1976 to put youth back to work at the minimum wage, the lowest acceptable wage, and the prevailing earnings of youth, both by using data for 1969 and 1973 as the benchmarks. As shown, paying the 2.7 million unemployed youths the same earnings as their employed counterparts would have been the most expensive way to lower the youth jobless rate. Using this wage scenario--and all the other assumptions that had to be made--we estimated that it would have cost $8.9 billion to reach the 1969-based goals and $6.0 billion to reach the 1973-based goals. On the other hand, under the minimum wage scenario it would have cost $5.7 billion to lower the 1976 incidence of youth joblessness to 1969 levels and $3.8 billion to lower it to the 1973 levels.

A closer examination of the youth wage bill under each different wage-earnings scenario can be made from Table 8, where the costs based on the various assumptions are broken down by age-sex group. It is not too surprising to see that the greatest cost would be incurred in providing jobs to workers age twenty to twenty-four (especially males). Such workers are most likely to be receiving higher wages than teenagers and to be seeking full-time jobs. For all workers aged twenty to twenty-four the wage bill would have amounted to $7.0 billion under prevailing wages and $4.0 billion if the minimum wage were used as the standard. In contrast, even under the prevailing earnings assumption, the cost of putting teenagers sixteen to nineteen to work would have

## TABLE 7.

## ESTIMATED COSTS OF PUTTING UNEMPLOYED YOUTHS TO WORK IN 1976 ACCORDING TO VARIOUS ASSUMPTIONS ABOUT THE INCIDENCE OF YOUTH UNEMPLOYMENT AND THE WAGE RATES FOR YOUTHS

| Number of unemployed and wage rate assumptions | Actual data for 1976 | Hypothetical data for 1976 based on 1969 incidence of unemployment | Job deficit and cost (1) - (2) | Hypothetical data for 1976 based on 1973 incidence of unemployment | Job deficit and cost (1) - (4) |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Number of persons 16 to 24 with some unemployment during year (in thousands)--- | 8,524 | 5,806 | 2,718 | 6,658 | 1,866 |
| Costs of wage bill (in millions of dollars) at: | | | | | |
| Prevailing earnings------- | $14,776.9 | $5,830.5 | $8,946.4 | $8,830.4 | $5,973.5 |
| Lowest acceptable wage---- | 12,104.4 | 4,641.1 | 7,463.3 | 7,130.2 | 4,974.2 |
| Minimum wage-------------- | 9,391.9 | 3,715.9 | 5,676.1 | 5,617.9 | 3,774.1 |

## TABLE 8.

## REDUCTION IN YOUTH UNEMPLOYMENT AND ITS COSTS IN 1976 UNDER VARIOUS EARNINGS AND WAGE ASSUMPTIONS

| Groups | Reduction in unemployment (000) | Prevailing earnings (000,000) | Lowest acceptable wage (000,000) | Minimum wage (000,000) |
|---|---|---|---|---|
| Assuming the 1969 incidence of youth unemployment | | | | |
| Total youth | 2,718 | $8,946.4 | $7,463.3 | $5,606.1 |
| Teenagers, 16 to 19 | 709 | 1,914.6 | 1,872.8 | 1,695.8 |
| Males | 301 | 1,115.5 | 1,117.2 | 986.4 |
| Females | 408 | 799.1 | 715.6 | 709.4 |
| Young workers, 20 to 24 | 2,010 | 7,031.8 | 5,630.5 | 3,980.2 |
| Males | 1,268 | 5,182.7 | 4,242.9 | 2,733.5 |
| Females | 742 | 1,849.0 | 1,387.6 | 1,246.7 |
| Assuming the 1973 incidence of youth unemployment | | | | |
| Total youth | 1,866 | 5,973.5 | 4,974.2 | 3,774.1 |
| Teenagers, 16 to 19 | 577 | 1,282.9 | 1,241.1 | 1,142.3 |
| Males | 304 | 758.2 | 799.6 | 704.6 |
| Females | 273 | 524.7 | 441.5 | 437.7 |
| Young workers, 20 to 24 | 1,293 | 4,690.6 | 3,733.1 | 2,631.8 |
| Males | 851 | 3,475.4 | 2,841.3 | 1,830.5 |
| Females | 442 | 1,215.3 | 891.8 | 801.2 |

amounted to only $1.9 billion, a figure not much higher than that obtained through minimum wage assumptions.

However, as already noted, the minimum wage may not be sufficiently high to attract many of the unemployed youths into jobs, especially those aged twenty to twenty-four. Undoubtedly, many would refuse to take minimum wage jobs, as their lowest acceptable wage is considerably above the minimum wage.

## LIMITATIONS AND QUESTIONS

The estimates we have presented should be regarded as merely illustrative as they are subject to many limitations. First of all, they relate to the employment situation for youths during 1976. More current estimates, and even better estimates derived on the basis of projections for future years, would, needless to say, be much more useful for policy guidance. However, 1976 was the last year for which "work experience data" were available and the only recent year in which reservation wages were collected. In order to make such estimates for future years one must not only project the size and distribution of youth unemployment but also make some assumptions about the future course of wages, and this is simply too difficult a task. (For those who would like to attempt such projections, we might indicate two elements whose future course is sufficiently well known: the minimum wage, which has been legislated to rise by specific amounts on specific dates over several years, and the size of the youth population, which is now increasing at a much slower pace than has been the case for the past decade and which will begin to decline rapidly during the early 1980s.)

As already noted, these cost estimates were based on the assumption that short-duration unemployment could be alleviated through job programs as effectively as longer-duration unemployment. This is, admittedly, a very questionable assumption, which we made only with the knowledge that the estimates could be easily reconstructed to exclude the short-duration unemployed from the calculations.[6]

Another obvious limitation of these cost estimates has to do with the quality of the earnings data used as one of the principal ingredients in the estimating procedure. Except for the minimum wage figure, the earnings data we have used have been obtained through the Current Population Survey, and they are subject to response bias as well as to the normal variance associated with such sample surveys. Recent studies have indicated that these household data tend generally to underestimate the true earnings of the workers in question, with the overall averages ranging from 3% to 5% lower than they would be if based on actual payroll records.[7] However, no findings have yet been derived from these studies with regard to the quality of the earnings data for youths. Furthermore, since the "prevailing earnings" statistics for the unemployed were developed through regression analysis, they may be subject to further statistical error and potential bias associated with this estimating device. (And here it might be noted that the even lower reliability of the subnational data on earnings, combined with the paucity of information on the distribution of unemployment at the local area level, discouraged us from even attempting to make any geographically oriented cost estimates.)

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[6] We experimented with such a procedure and calculated that if the goal in 1976 had been to place all unemployed youths in job programs after they had accumulated 4 weeks of unemployment, the wage costs would have been $7.2 billion on the basis of the Federal minimum, $9.3 billion on the basis of the lowest acceptable wages, and $11.4 billion on the basis of the prevailing earnings. Of course, these calculations are based on the assumption that the youths would have remained in these job programs for only as long as they would have remained unemployed if they had not taken these jobs, and this is another questionable assumption.

[7] These are the findings from a "validation" study conducted in January 1977, when the data obtained from the Current Population Survey on earnings of about 4,000 workers were subsequently checked against the records of their employers. While the detailed report on this study has not yet been published, a discussion of the quality of the earnings data derived from the household survey had appeared in a previous BLS report. See Special Labor Force Report No. 195 "Weekly and Hourly Earnings Data from the Current Population Survey", U.S. Department of Labor, Bureau of Labor Statistics, 1977.

It is also important to note, with regard to the cost estimates discussed above, that they are limited purely to the wages that would have to be paid and do not make any allowances for fringe benefits or administrative expenses. Yet we know that such costs would be substantial. In 1974, for example, fringe benefits of various kinds raised the total costs of labor to employers by one third over the pure cost of straight time wages.[8] And while it would be reasonable to assume that the employment of youths would not have to be accompanied by the payment of fringe benefits as costly as those that would generally go to older workers with high seniority, it must be noted that payments for Social Security, workers compensation, and so forth must be made by employers even in the case of newly hired youths--and these payments are not necessarily negligible. As for the administrative costs of running youth job programs, this is an additional cost that must be taken into account but which can probably be better estimated by administrators than by economists unfamiliar with administrative machinery.

Of course, offsetting all these costs, there would also be some obvious benefits that would flow from the employment of large numbers of youths. Presumably, production would increase; disposable income would rise; some youths might be diverted from less desirable endeavors, and so on. But we know of no way even to begin to put a dollar estimate on this side of the ledger.

Finally, one might ask whether the creation of a large number of jobs for youths, particularly if at wage levels that most youth would find quite acceptable, could wind up in attracting many additional youths into the labor market and thus vitiate the efforts to lower their unemployment rates. Given the relatively low labor force participation rates of some groups within the youth population--black youths, for example--such a possibility could not be ruled out. There is, in fact, some evidence that this would be the case.[9]

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[8] _BLS Measures of Compensation_, U.S. Department of Labor, Bureau of Labor Statistics, 1977, Bulletin 1941, p. 2 .

Should such a situation arise, one objective way to assess the impact of job programs would be to focus on the employment-population ratios of the affected groups. After all, if the employment-population ratio for black youths were to remain much lower than that for white youths, as is now the case, something would still be seriously amiss even if their unemployment rate drops sharply. Conversely, an increase in their employment-population ratio should be regarded as a favorable development even if their unemployment rate fails to recede significantly.

## SUMMARY AND CONCLUSIONS

In this paper we have examined the issue of how low the unemployment rate for youths might be brought, and pointed out that for the entire sixteen to twenty-four group the rate has never averaged less than 8% for any year since the Korean War. In addition, we indicate that in some recent "full-employment" estimates the youth jobless rates have been pegged even higher.

With regard to the wages that would have to be paid to youth placed in job programs, we presented several distributions, all of which show a large dispersion. Many youths, particularly in the sixteen to nineteen group, are apparently willing to work for less than the federal minimum. At the same time, many others, particularly those twenty to twenty-four, are generally earning much more--and expect to earn much more--than the minimum.

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[9] See, for example, Stanley L. Friedlander, Unemployment in the Urban Core: An Analysis of Thirty Cities with Policy Recommendations. (New York: Praegar: 1972). Reference to the increase in the supply of labor resulting from the creation of a given number of jobs can be found on page 133.

Finally, we used the averages from these distributions, along with the federal minimum, to compute some estimate of what it might have cost in 1976 to place enough unemployed youths in jobs so as to lower their incidence of unemployment to the levels of 1969 and, alternatively, to those of 1973. Although these estimates are based on many assumptions and are subject to many limitations, they illustrate the potential of the demographically-oriented earnings data which have become available in recent years.

## TECHNICAL NOTE

### SOURCES OF DATA

The cost estimates presented in this paper were developed from data obtained from the Current Population Survey (CPS) conducted by the Bureau of the Census. Specifically, the statistics on youth unemployment during 1969, 1973, and 1976 were collected in the March 1970, March 1974, and March 1977 surveys. These data, along with the employment and unemployment data for other workers, appeared in various issues of the Bureau of Labor Statistics' Special Labor Force Reports (Report #127, #171, and #201). However, some unpublished data from these surveys were also used in the calculations. With respect to the wages and earnings used in the calculations, the lowest acceptable wages of youth were collected in a special CPS survey of the unemployed taken in May 1976 (these data have never before been published); the prevailing earnings of the unemployed were developed through regression analysis that used the usual weekly earnings of employed workers also collected in the May 1976 CPS (see footnotes 3 and 6 of text for specific references).

### METHODOLOGY

The general principle involved in estimating the costs of youth unemployment consisted of multiplying the number of person-weeks, or person-hours, of youth unemployment by first, the minimum wage ($2.30 in 1976), second, the lowest acceptable hourly wages of youth, and, third, the prevailing earnings of unemployed youth.

All of the estimates were first developed for eight groups of youth (men, sixteen to nineteen, looking for full-time jobs; men sixteen to nineteen, looking for part-time jobs; women sixteen to nineteen, looking for full-time jobs; women sixteen to nineteen, looking for part-time jobs; and the four corresponding groups of workers age twenty to twenty-four). These estimates were then summed to yield the total cost estimates. The specific steps followed in making these estimates were as follows:

Step 1. Estimating the total costs of youth unemployment in 1976. For each of the eight groups of youth mentioned above, estimates were made of the number of person-hours and person-weeks lost through unemployment in 1976. The number of person-hours lost was used to prepare the cost estimates using the minimum wage and the lowest acceptable wage; the number of person-weeks lost was used in making the estimates based on the potential weekly earnings of the unemployed.

The extent of youth unemployment in 1976 was tabulated in weekly intervals (1 to 4 weeks, 5 to 10, 11 to 14, 15 to 26, 27 to 40, and 40 or more weeks). It was assumed that youths classified in each interval experienced the number of weeks of unemployment represented by the mid-point of each interval (ie. the mid-point of the 1 to 4 weeks interval was 2.5 weeks). Since these calculations yielded estimates of the number of person-weeks of unemployment for each of the eight groups of youth, it was now possible to multiply them by the potential weekly earnings of the unemployed to arrive at one of the three total cost estimates.

To produce the cost estimates based on the minimum wage and lowest acceptable hourly wage, it was necessary to convert the person-weeks of youth unemployment into person-hours. Using forty hours for those seeking full-time work and the actual annual average number of part-time hours worked by youth, person-hours of unemployment for youth in 1976 were estimated. The minimum wage and the lowest acceptable hourly wages were then multiplied by these person-hours to yield the other two total cost estimates of youth unemployment.

Step 2. Estimating the job deficit. To estimate the number of jobs that would have been required to reduce youth joblessness in 1976 to the rates experienced in 1973 and 1969, the following procedure was used: The proportions of the youth population (civilian noninstitutional population) in 1969 and 1973 who experienced some unemployment during these years were applied to the youth population in 1976. The products of these multiplications were then subtracted from the actual 1976 level of youth unemployment to arrive at the size of the job deficits.

Obviously, we have assumed that the proportions of youths experiencing unemployment during 1969 and 1973 are consistent with the rate, or incidence, of youth unemployment in these years.

These hypothetical levels of unemployment for 1976, based on the 1969 and 1973 experiences, were then used to calculate two alternative total costs of youth unemployment.

Step 3. Estimating total costs of youth unemployment in 1976 under two different assumptions about the level and distribution of youth unemployment. To prepare total cost estimates of youth unemployment under two different assumptions about its level and distribution, it was necessary to return to the actual data for those years. The actual percent distributions of youth unemployment were calculated (the eight youth groups mentioned above, cross-classified by weeks of unemployment, comprised the distribution) for both 1969 and 1973. (The same procedure as in Step (1), was used in developing the distribution of person-hours of unemployment.) These two percent distributions were then applied to the two hypothetical levels of unemployment estimated in Step (2). Once the actual numbers of person-weeks and person-hours had been calculated, they were multiplied by the minimum wage, lowest acceptable wages, and prevailing earnings of youths to arrive at the total costs estimates.

Subtracting these two sets of total costs from the actual total costs of youth unemployment in 1976, yielded the costs involved in bringing down youth unemployment to rates experienced in 1969 and 1976.

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